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[FOURTH SERIES.]

ART. XIX.—*Principal Characters of the Protoceratidæ;* by O. C. MARSH. Part I. (With Plates II–VII.)

THE genus *Protoceras*, described by the writer in 1891, from the Miocene of South Dakota, is now known to include some of the most interesting extinct mammals yet discovered. It likewise represents a distinct family, and thus deserves careful investigation and description.* Before this discovery, no horned artiodactyles were known to have lived during Miocene time, and *Protoceras* is thus the earliest one described. The type specimen, moreover, had a pair of horn-cores on the parietals, and not on the frontals as in modern forms of this group. The animal was apparently a true ruminant, nearly as large as a sheep, but of more delicate proportions.

The first skull found, the type specimen of the genus *Protoceras*, belonged to a female, as later discoveries demonstrated. The skull of the male proved still more remarkable, and especially resembles the male skull of the Eocene *Dinocerata* in having several pairs of horn-cores or protuberances upon the head, a feature hitherto unknown among the *Artiodactyla*. It is an interesting fact, moreover, that one pair of these horn-cores of *Protoceras* is on the maxillaries, as in *Dinoceras*, while the posterior pair, as in that genus, is on the parietals.

* This Journal, vol. xli, p. 81, January, 1891; and also, vol. xlvi, p. 407, November, 1893.

The resemblance in the two skulls is further enhanced by the absence of upper incisors and the presence of large canine tusks, forming together a striking similarity in important features, between skulls pertaining to animals of two distinct orders, and from widely different geological horizons. The skull of the male *Protoceras* is shown in Plate II, and that of *Dinoceras* in the text below.

1.

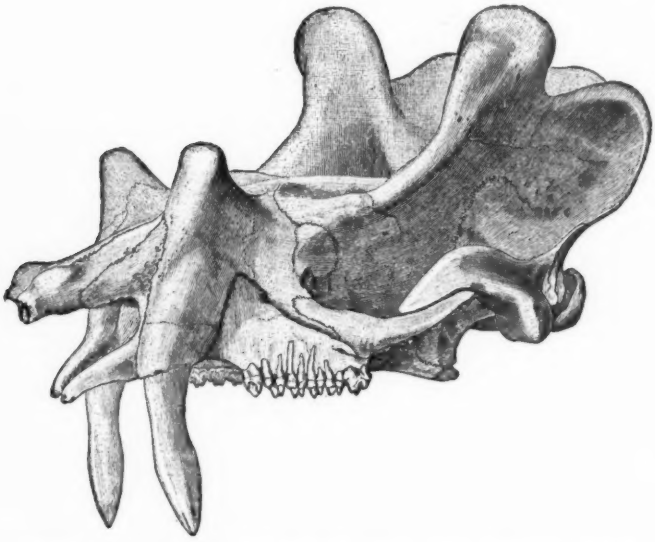


FIGURE 1.—Skull of *Dinoceras mirabile*, Marsh; type; seen from the side. One-seventh natural size. Eocene.

It is a noteworthy fact, that in still another order of ungulate mammals, the *Perissodactyla*, horn-cores in pairs early made their appearance, although none are known in the recent forms. One of the earliest instances is seen in the genus *Colonoceras* from the middle Eocene, which had rudimentary protuberances upon its nasal bones, as represented below, in figure 2. The gigantic *Brontotheridæ* of the lower Miocene all had prominent horn-cores on the maxillary bones, somewhat like those of the male *Protoceras*. One of the most unexpected examples, however, in this order, appears in the Miocene genus *Diceratherium*, the type specimen of which is shown in figure 3. This animal, although a true rhinoceros, had a pair of horn-cores on the nasal bones, while all other rhinoceroses, living and extinct, are either without horns or have them on the median line. In short, horns in pairs are unknown in existing mammals, except in the artiodactyles, an order of later development, but now the dominant group of ungulate mammals.

The Male Skull.

The skull of the male *Protoceras*, in addition to the marked characters above mentioned, has others of equal interest, if not of still greater taxonomic value.* The general appearance of the adult male skull is well shown in Plate II, accompanying the present article, and the special anatomical characters are represented more clearly in the different views on Plates III, IV, V, and VI.

2. 3.

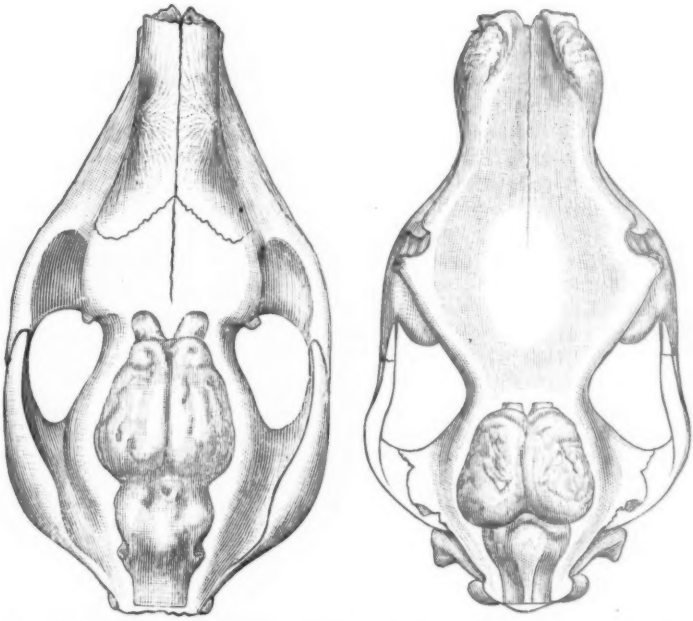


FIGURE 2.—Skull of *Colonoceras agrestis*, Marsh; type, with brain cast; seen from above. About one-half natural size. Eocene.

FIGURE 3.—Skull of *Diceratherium advenum*, Marsh; type, with brain cast; seen from above. One-sixth natural size. Miocene.

Aside from the various horn-cores and protuberances upon the skull, the next most notable feature is the very large, open nasal cavity, a character which pertains to both sexes, and to the entire family of the *Protoceratidæ*. This peculiar feature is of even more importance than the horn-cores, judging from its functional significance, and its rarity in more recent forms of artiodactyles. It indicates clearly in the living animal a

* Osborn and Wortman, Bulletin, Amer. Mus. Nat. Hist., vol. iv, p. 351, 1892. See also Scott, Jour. Morph., vol. xi, p. 303, 1895.

long flexible nose, if not a true proboscis. The only existing ruminant thus equipped, known to the writer, is the rare Saiga antelope (*Saiga Tartarica*, Gray) from the steppes of Siberia. A comparison of a *Protoceras* skull with that of the Saiga antelope plainly indicates, in the nasal region, an identity of function doubtless accompanied by a similar nasal appendage, and it is of interest to find such evidence of this feature in a representative from the Miocene of North America.

The general form of the male skull of *Protoceras* is long and narrow, with the facial portion much produced. The prominent horn-cores, however, serve to obscure its real shape, which is more apparent in the female skull. Seen from the side, as in Plate III, it appears unusually low, with the orbit well behind. Its greatest width is in the posterior region, as shown in Plates V and VI.

The premaxillaries are small and edentulous. Their anterior extremities are depressed, and more or less expanded transversely, as in typical ruminants. The outer suture between the premaxillary and maxillary is short, and persistent even in adults, as indicated in Plates II and III. Seen from below, the premaxillaries form together the palatal surface in front of the maxillaries, each sending backward a narrow process which is inserted between the divergent maxillary plates. The anterior palatine, or incisive, foramina are situated on the sutures separating the two bones, as represented in Plate V.

The maxillary bones are greatly developed, being much the largest elements of the skull, as is well shown in Plate II. The anterior extremity supports the large descending canine tusk, and is hollowed out to contain its base. The high anterior horn-cores are formed entirely of the maxillary bones, which are greatly strengthened to support them. These horn-cores are more or less recurved, and in the type species, their summits are triangular in outline, as seen in Plates II and III, and in the cut below, figure 4. In a new species, *Protoceras nasutus*, the summits of the maxillary horn-cores are oval in section, as shown in cut 5. Another characteristic feature of the genus *Protoceras*, which is seen in both sexes, is a strong lateral ridge extending nearly horizontally across the outer face of the maxillary bone, and continuing backward to the orbit. It is also present in the other members of this family. In the male skull here described, this ridge begins near the base of the maxillary horn-core, and, expanding into a prominent tubercle, just above the antorbital foramen, continues backward by an upward curve, and passes into the ridge of the malar bone extending beneath the orbit. In both sexes, the anterior portion of this lateral ridge, with its characteristic tubercle, forms the lower border of a deep, well-marked

depression, which probably contained a gland. In Plate II, this cavity is well shown behind the maxillary horn-cores, just below the point where the superior border of the skull is lowest.

The nasal bones join the maxillaries above, and complete the posterior border of the large narial opening. They are of moderate length on the median line, and their free anterior extremities are quite short. These bones are much expanded transversely, and at their widest part articulate with the lachrymals. All the sutures surrounding the nasals are distinct, and this is true, also, of their median suture. Their upper surface is convex, both transversely and longitudinally, and is marked by two deep grooves, which lead backward to the supra-orbital foramina in the parietals, as shown in Plate IV.

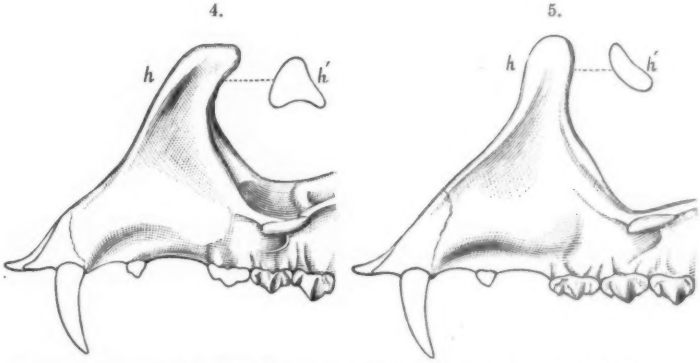


FIGURE 4.—Front of skull of *Protoceras celer*, Marsh; seen from the left side.
FIGURE 5.—Front of skull of *Protoceras nasutus*, Marsh; seen from the left.
Both are male skulls, and drawn one-half natural size. Miocene.
h, maxillary horn-core; *h'*, section of same.

The frontals, which bound the nasals behind, are large massive bones, much wider than long. The suture which unites the two frontals is distinct, and cuts the naso-frontal suture nearly at right angles. At the lateral junction of the frontal and nasal, there is on each side a low tuberosity, resembling a diminutive horn-core, and these form the third pair of elevations on the skull. At the postero-external angle of the frontals, above the orbits, another pair of much larger protuberances is seen, and the summits of these are widely expanded transversely, as shown in Plate IV. The upper surface of the frontals is rugose, and the deep grooves already mentioned are characteristic features.

The parietal bones are much smaller than the frontals, and are separated from them by a distinct sigmoid suture. These bones support the posterior pair of horn-cores, as shown in Plate IV. The general form and position of these elevations on the male *Protoceras* skull are represented in the accompanying plates, but they differ in each species. Behind these horn-cores, there is a low sagittal crest separating the deep temporal fossæ. Back of the parietals is the short supra-occipital, which forms a weak lambdoidal crest bounding the temporal fossæ behind.

The inferior portion of each fossa is formed by the squamosal, which covers the lower half of the brain case, and joins the parietal above by a distinct suture, as shown in Plate III. The squamosal sends forward a short zygomatic branch, which fits into a notch in the posterior part of the malar. There is a distinct postglenoid process. The tympanic bone is not dilated into a definite bulla, but below the auditory meatus forms a short descending process. The periotic is behind the tympanic, separated from it above by the post-tympanic process of the squamosal, and below by an open suture. It is wedged in between the latter bone and the strong and elongate paroccipital process of the exoccipital.

The orbit is closed behind by a descending process from the frontal, which meets the upper branch of the malar. Its lower border is bounded by the malar, which in front joins the lachrymal above and the maxillary below, as shown in Plates II and III.

The lachrymal is bounded in front by the maxillary, above by the nasal and frontal, and below by the malar and maxillary. The lachrymal foramina are two in number, well within the orbital border. The orbits are large, suboval in outline, and widely separated from each other. Their posterior position is a characteristic feature of the genus *Protoceras*.

The Base of the Skull.

The lower surface of the male *Protoceras* skull is represented in Plate V. The narrow occiput, surmounted by the supra-occipital, is a noteworthy character. The widely expanding orbits greatly increase the width of the skull in this region, and from here forward, its wedge-like shape is a striking feature. The large foramen magnum and the narrow diverging occipital condyles are well seen in this view. The basi-occipital and the basisphenoid bones are firmly coössified, the suture between them being indistinct. In front of the latter bone is the parasphenoid, separated from it by a well-marked suture, and passing forward above the vomers, which are here distinct. The pterygoids are attached to the posterior border of the palatines, and above to the alisphenoids. There is no distinct alisphenoid canal.

The palatine bones are narrow, and bound in front the posterior nares, which extend forward to near the middle of the penultimate molars. The maxillary plates form the roof of the palate forward to the premaxillaries. At their narrowest portion, they are deeply grooved for the approaches of the palato-maxillary foramina, which are situated somewhat in advance of the second premolars. The maxillary plates are separated in front along the median line, to receive the posterior branches of the premaxillaries, and on the suture between the two elements, the anterior palatine foramina are in their usual position. The turbinal bones were apparently quite small.

The Lower Jaw.

The lower jaw is well represented in Plate II. It is long and slender, especially in front, thus corresponding to the skull. The condyle is broad and strongly convex above. The coronoid process is very short, and its summit is but little higher than the condyle. The angle is rounded and well developed. The ramus expands downward and is thickened beneath the molar teeth, and has a sharp upper edge along the diastema between the first and second premolars. It again extends downward at the symphysis, becoming more robust to support the front teeth.

The Dentition.

The dentition of *Protoceras* is of the early ruminant type, as shown by the short-crowned, selenodont molar series. The dental formula is as follows:

$$\text{Incisors } \frac{0}{3},\ \text{Canines } \frac{1}{1},\ \text{Premolars } \frac{4}{4},\ \text{Molars } \frac{3}{3}.$$

In the male skull, the upper canines are well developed, as shown in Plate II. They are compressed and somewhat trihedral in transverse section, and in life formed efficient weapons of warfare. The first upper premolars, a short distance behind, are small compressed teeth, each with two roots; and after a still longer diastema, the second premolars begin the continuous series. The second and third upper premolars each have a large outer cusp and an inner cingulum, while the fourth has a distinct inner crescent, as shown in Plate V, which also represents faithfully the superior molars. These have all short crowns and the double crescents of true selenodont dentition, with a well-developed inner basal ridge on each. The accurate drawings of the accompanying plates render unnecessary a detailed description of these teeth and most of the others here figured. This is true, also, of various minor points in the structure of the skull.

The teeth of the lower jaw of *Protoceras* are indicated in Plate II, and the full series is shown. The three incisors are directed well forward, and diminish in size from the first to the third. The still smaller canine is situated close to the last incisor, and is similar in form. A long diastema follows, and gives the upper canine freedom of motion. The first premolar is somewhat similar to the corresponding one above, but is larger and directed more forward. A still longer interval separates the first and second lower premolars, the latter beginning the continuous molar series. The second premolar has the crown much compressed, while the third and fourth are triangular in form. The three true molars have the usual crescents corresponding to those above, but no inner cingulum.

The upper molar teeth of the female skull are shown in figure 7, below, which represents the type of the genus. On Plate VI, figure 2, the upper dentition of *Protoceras comptus* is represented, the type specimen figured being the skull of a female not yet adult.* The last three deciduous teeth are here still in use, the first and second true molars are in position, while the last had not yet come into place.

The Brain.

The brain in *Protoceras* was of good size, not diminutive as in the early ungulates. It was, moreover, well convoluted for a Miocene mammal, and forms an interesting addition to our knowledge of the brain development in Tertiary *Mammalia*.

The natural brain cast figured in Plate VII, figures 3 and 4, is from an adult female skull, and represents accurately the brain cavity of this individual, except the small space occupied by the olfactory lobes. The latter were well developed.

The Female Skull.

The type species of the genus *Protoceras*, as already stated, was the skull of a female, and it may be well to repeat here its essential features as given in the original description already cited. In figures 6 and 7 below, most of the main characters of this type specimen are represented.

"In general form and proportions, this skull is of the ruminant type. Its most striking feature is a pair of small horn-cores, situated, not on the frontals, but on the parietals, immediately behind the frontal suture. These prominences were thus placed directly over the cerebral hemispheres of the brain.

* This Journal, vol. xlviii, p. 93, July, 1894.

"The frontal bones are very rugose on their upper surface, and this rugosity extends backward on the parietals, and to the summit of the horn-cores, as well as between the latter, and along the wide sagittal crest. The horn-cores are well separated from each other, and point upward, outward, and backward, overhanging somewhat the temporal fossæ. They are conical in form, with obtuse summits.

"Between the orbits, the frontals are depressed, and marked by two deep grooves leading backward to the supra-orbital foramina. Behind these, halfway to the horn-cores, is a median prominence resembling in shape the corresponding elevation on the skull of the male giraffe. The brain cavity is unusually large for a Miocene mammal. The occiput is very narrow, indicating a small cerebellum, and the occipital crest is weak. The occipital surface slopes backward.

6. 7.

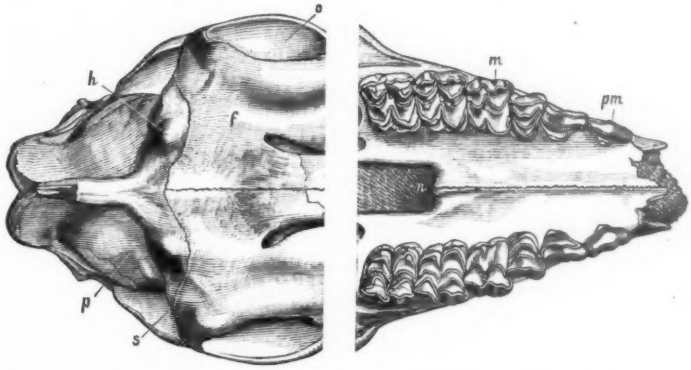


FIGURE 6.—Back of female skull of *Protoceras celer*; type; seen from above.
FIGURE 7.—Front of same skull; seen from below.

Both figures are one-half natural size. Miocene.

f, frontal; *h*, horn-core; *m*, first molar; *n*, posterior nares; *o*, orbit; *p*, parietal; *pm*, second premolar; *s*, suture between frontal and parietal.

"The facial region of the skull is narrow and elongate. On the outer surface of the maxillary, just above the antorbital foramen, there is a deep depression, which probably contained a gland. The usual ruminant fossa in front of the orbit appears to be wanting. The orbit is large, and completely closed behind by a strong bar of bone.

"The dentition preserved is selenodont and brachyodont, with only three premolars and three molars.* The first premolar is much compressed transversely, and has but a slight inner lobe. The second premolar is triangular in outline, the inner lobe being much more developed. The last premolar has this lobe expanded into a strong cusp, and the crown thus becomes broader than long. The true molars have two inner cusps, each with a basal ridge. The outer crescents have a median vertical ridge. The enamel of the molar series is more or less rugose. There was a wide diastema in front of the premolars.

"The posterior nares are situated far forward, the anterior border being opposite to the posterior cusp of the second true molar. The glenoid facet is large and convex, but the post-glenoid process is quite small. The paroccipital processes were well developed, but there were apparently no auditory bullæ."

A number of other female skulls, some of them in excellent preservation, have since been obtained from the same region in which the type was found, and a study of these makes clear the main points of their structure. It is not quite certain to which of the three species of *Protoceras* now known some of these skulls should be referred, but further investigation will doubtless determine this point, as the present material in the Yale Museum is apparently sufficient for this purpose.

The Skull of Calops.

The small artiodactyle described by the writer in 1894, under the name *Calops cristatus*, is from essentially the same geological horizon in South Dakota in which *Protoceras* was found. As stated in the first description, *Calops* possesses characters indicating a near ally of *Protoceras*, and as the resemblance has proved even closer in more perfect specimens since discovered, denoting that the two genera belong to the same family, it may be well to quote here the main points of the original description.†

"The type specimen is a skull in fair preservation, indicating a fully adult animal, which when alive was about half as large as a goat. In its general form and in most of its characters, this skull agrees so closely with the type of *Protoceras* as to suggest at once some affinity between the two. The dentition preserved in the premolar and molar series is essentially the same. The high maxillary plates joining the short, pointed nasals; the deep lachrymal fossa; and the posterior orbit

*More perfect specimens since discovered prove that there were four premolars, the first being absent in the type.

†This Journal, vol. xlviii, p. 94, July, 1894.

strongly closed behind, all suggest an ally of *Protoceras*, but the parietal ridges are here elevated into distinct crests, and are without horns.

"This skull when complete was about six inches in length. The distance from the front of the nasals to the junction of the parietal crests is about four inches and a half. The space occupied by the last three premolars and the true molars is about two and one-half inches."

In a later notice, a second more perfect specimen from the same horizon was described,* the main points stated being as follows:

"The brain was comparatively well developed, and an unusually large part of the cerebral lobes was covered by the parietals. The frontal region of the skull between the orbits was more or less concave. The antorbital depressions extend well forward. There is a diastema between the upper canine and the first premolar, and between the first and second premolars. The canines above and below are small. The first lower premolar appears to be wanting. The second and third premolars have secant crowns, much elongated fore and aft. The postglenoid process is quite small, but the paroccipital is large and robust. The lower jaw has a very short coronoid process, and the condyle is sessile. The angle of the jaw is well rounded and somewhat dependent."

This second specimen proves to be distinct from the type, and is here recorded as a new species, *Calops consors*. The skull, which is in good preservation, is represented on Plate VII, figures 1 and 2. These two views exhibit the main features of the skull in the genus *Calops*. The most striking difference between this specimen and the type is the position of the orbit, which in the latter is entirely behind the molar series, as in *Protoceras*, while in the specimen here figured, as shown in Plate VII, nearly half the orbit is in front of the posterior end of this series.

The Dentition of Calops.

The teeth of *Calops* correspond essentially with those of *Protoceras*, being of the same early ruminant type, with the characteristic, short-crowned, selenodont molar series, and apparently the same dental formula. In the female skull represented on Plate VII, figure 1, most of the teeth are seen in position. There were no upper incisors. The canine was of moderate size, and placed well back of the premaxillary suture. The first premolar is small, with a compressed crown and two roots, and is situated somewhat behind the middle of the

* This Journal, vol. xlviii, p. 273, September, 1894.

interval between the canine and second premolar, as shown in the figure cited. The remaining upper premolars correspond closely with those of *Protoceras* in form, and this is true, also, of the molars. The lower incisors of *Calops* are small and procumbent. The canine also was small, and probably similar in form to the incisors. The first lower premolar is caniniform in shape, with a single root, and a sharp compressed crown, which came nearly in apposition to the superior canine. The remaining lower premolars and molars agree closely except in size with those of *Protoceras*.

The remains of *Calops* now known all appear to have pertained to females, and this naturally suggests the question—what the male skull was like, and especially whether it was provided with horns. The probabilities at present are in favor of the latter view, but it must be left to future discoveries to settle that point.

All the known remains of *Protoceras* and *Calops* are from the upper Miocene of South Dakota. The horizon, which is a definite one, has been appropriately called by Dr. Wortman the Protoceras beds. They appear to be identical with the series in Oregon which the writer had previously named the Miohippus beds, as that genus and several others are common to both regions.

Yale University, New Haven, Conn., July 24, 1897.

EXPLANATION OF PLATES.

PLATE II.

Male skull, with lower jaw, of *Protoceras celer*, Marsh; oblique side view. Three-fourths natural size.

PLATE III.

The same skull; seen from the left side. Three-fourths natural size.

PLATE IV.

The same skull; seen from above. Three-fourths natural size.

PLATE V.

The same skull; seen from below. Three-fourths natural size.

PLATE VI.

FIGURE 1.—The same skull; seen from in front.
FIGURE 2.—Front of skull of *Protoceras comptus*, Marsh; seen from below young female, showing deciduous dentition.

Both figures are three-fourths natural size.

PLATE VII.

FIGURE 1.—Skull, with lower jaw, of *Calops consors*, Marsh; seen from the left.
FIGURE 2.—The same skull; seen from above.
FIGURE 3.—Natural brain cast of *Protoceras celer;* female; side view.
FIGURE 4.—The same; seen from above.

The figures are all one-half natural size.

[To be continued.]

ART. XX. — *The Theory of Singing Flames;* by H. V. GILL, S. J.

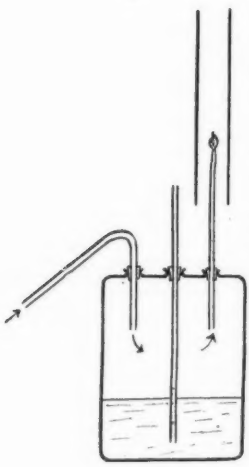
I.

THE phenomenon of a jet of gas burning inside an open tube, emitting a musical note, is one of those facts which, although known for very many years and although much written about, have never been fully explained. It is not our intention to go into historical details on the subject, but a glance at the chief explanations which have been proposed will be interesting. De la Rive supposed the sound to be due to a periodic condensation of the water vapor produced in the combustion of hydrogen gas. Faraday showed this theory to be false by the fact that he obtained a musical note by means of another gas which does not form water as a product of combustion. Faraday explained the sound as being produced by successive explosions of quantities of an explosive mixture of gas and air which succeed each other at certain intervals. Tyndall accepted this explanation. Another theory which has been proposed is that the sound is produced by vibrations maintained by heat, the heat being communicated periodically to the mass of air confined in the sounding tube, at a place where in the course of vibration the pressure changes. This explanation, although it takes into consideration the extinction of the flame at periodic intervals by the changes of pressure, is not satisfying, and indeed this very intermittent character of the flame presents in this theory certain serious difficulties.

Sondhauss performed a series of experiments in which he made use of a flame of hydrogen issuing from a gas-generating flask. His chief conclusion was that the condition of the column of gas in the supply-tube had an important influence on the phenomenon; for example, if the supply-tube be plugged near the jet with some wool the flame will not sing, though in appearance it is the same as a flame that will sing. This result is a proof that it is impossible to explain the singing by considering merely its effect in heating the air, as in the case of a wire gauze which has been heated. Rijke was able to produce a continuous musical note by means of a wire gauze placed in a tube and kept heated by means of a strong electric current. Now if the note produced by a gas flame were owing to the same cause, the flame, even when the supply-tube was plugged, ought to produce a note if placed at the same position as was the hot gauze when it was sounding, but no such result is observed even when the tube is narrow.

We have performed many experiments to verify the conclusions of Sondhauss, but we did not make use of a gas-generating flask to produce the gas. We employed ordinary coal gas, which passed into a flask* which was provided with two other tubes (fig. 1), one being in connection with the singing flame; the other had one extremity below the level of some water in the flask, the other end being open to the air, thus providing a means of measuring the pressure in the gas. With this arrangement we were able to obtain all the conditions of a gas-generating flask, with the additional advantage that we could regulate the size of the flame, etc., with perfect facility. It is evident that with a flame proceeding from a flask in which hydrogen is generated in the ordinary manner, it is almost impossible to regulate the gas supply with any exactness. All our experiments tended to show that the influence of the supply-tube came not from its length, but from the facility with which it allowed the gas to pass to the flame. For with the same supply-tube we were able to obtain any note, either high or low, which we desired, by modifying the size of the flame, its position in the tube, and the length of the tube in which it sang.

1.



In this paper we shall, we think, make it clear that the cause which plays the important part in the production of a musical note by the flame, is one whose effect has not been taken into account by those who have examined this question.

A brief review of the principal facts hitherto observed will be useful to us in what follows:

1st. The note produced depends on the length of the tube inside which the flame sings, on the size of the flame, and on its position within the tube.

2d. The notes are those proper to the tube, account being taken of the temperature of the air inside it.

3d. The flame must be smaller when it begins than when it is singing well.

4th. The spontaneous commencement does not seem to be an essential part of the phenomenon.

* There is no advantage gained from this arrangement in producing the singing flame. The supply-tube may be connected directly with the gas main of the house.

5th. When the flame is of such a size, and placed at such a position that it is on the point of beginning to sing, it may be made to begin by sounding the note proper to the tube; a sudden noise such as the clapping of the hands will sometimes suffice.

6th. The singing may be made to cease by closing one end of the tube, and sometimes by a sudden noise.

7th. Viewed in a rotating mirror the image is composed of a series of tongues, each tongue being separated from the others by a dark space.

8th. When the flame is too large to begin easily, it will respond to the note proper to the tube, but will only sound while the external note is sounding.

9th. The flame becomes blue and somewhat longer when it sings.

10th. Less gas is used when the flame sings than when it remains silent.

11th. If the flame be too small it will be extinguished in a few seconds by the violence of the action.

These are the chief facts which have been recounted by Tyndall and others; there are other facts known which may be looked on as deductions from those enumerated.

In the explanation we propose it will be seen that the theory of explosions, which is admitted by some even at the present time, is not the correct one. All the facts we rely on have been proved by actual experiment, and we make no hypothesis which has not experimental as well as theoretical corroboration.

As the spontaneous commencement is not an essential part of the phenomenon, we shall first examine the flame in the actual state of sounding, and shall then show how it begins.

A consideration of the conditions of pressure of the column of air in a sounding tube is the first step in our explanation.

When a tube, open at both ends, emits a musical note, the column of air divides itself up into nodes and loops or ventral segments. The position of the nodes depends on the note emitted, i. e., whether the tube emits the fundamental note, its octave, etc. At a node there is a considerable variation of the pressure, produced by the longitudinal vibration of the column of air. The pressure varies from its maximum during a condensation to its minimum during a rarefaction; these two conditions occurring in each complete vibration. Various methods are in use for demonstrating this fact, the best known being the manometric flames of Koenig. When such a flame is placed at a node, and its image observed in a rotating mirror, a band of light is seen from which arises a series of tongues, separated by dark spaces, each tongue corresponding to a condensation and each dark space to a rarefaction. Sometimes the

violence of these changes of pressure is so great that it extinguishes the flame altogether.

Though this method shows us that there is a considerable change of pressure, it does not give any numerical measurement. Such measurements have been made by Kundt and others. Kundt employed a water manometer for this purpose. As the changes of pressure follow each other very rapidly it is clear that an ordinary manometer would be useless, and hence he used one which, by means of a valve, could only be acted upon by changes of pressure of a given sign. With such an apparatus he found that, at a node of an open pipe sounding loudly, the increase of pressure during a condensation was equivalent to that exerted by a column of water about 15^{cm} high, and a diminution of equal amount during a rarefaction. Others have found lower values.

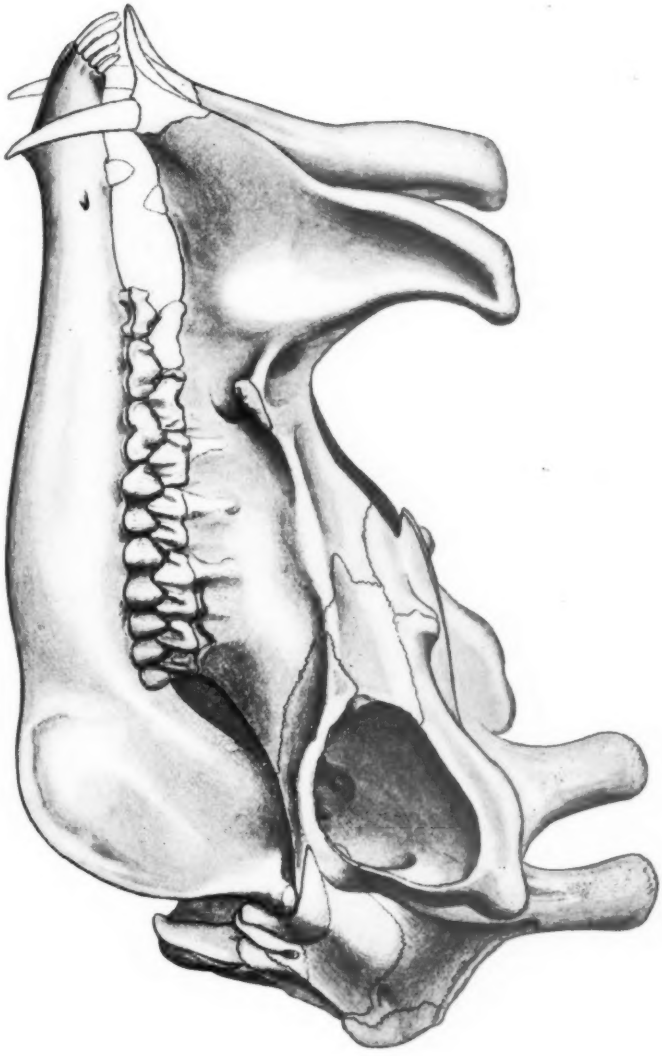
We have next to examine another pressure which comes into play in the case of the singing flame, one which has been altogether neglected by those who have proposed explanations of this phenomenon, but which we shall show to be an essential element in the production of the musical note.

The pressure of the gas which produces a flame of suitable size for a singing flame may be easily determined by means of the apparatus we have described. We have only to extinguish the flame which has been singing and close up the aperture. The water will then rise in the pressure-tube. This pressure is one, or two, or even more centimeters of water, according to the note which the flame produced. One might be inclined to think that the pressure under which the gas of the flame issues is always that on the gas supply of the house, but it is easy to show that when the tap which connects the flask to the main pipe is turned so as to let a small quantity of gas issue, that the pressure is proportional to the passage thus modified, the reason being the friction and viscosity of the gas.

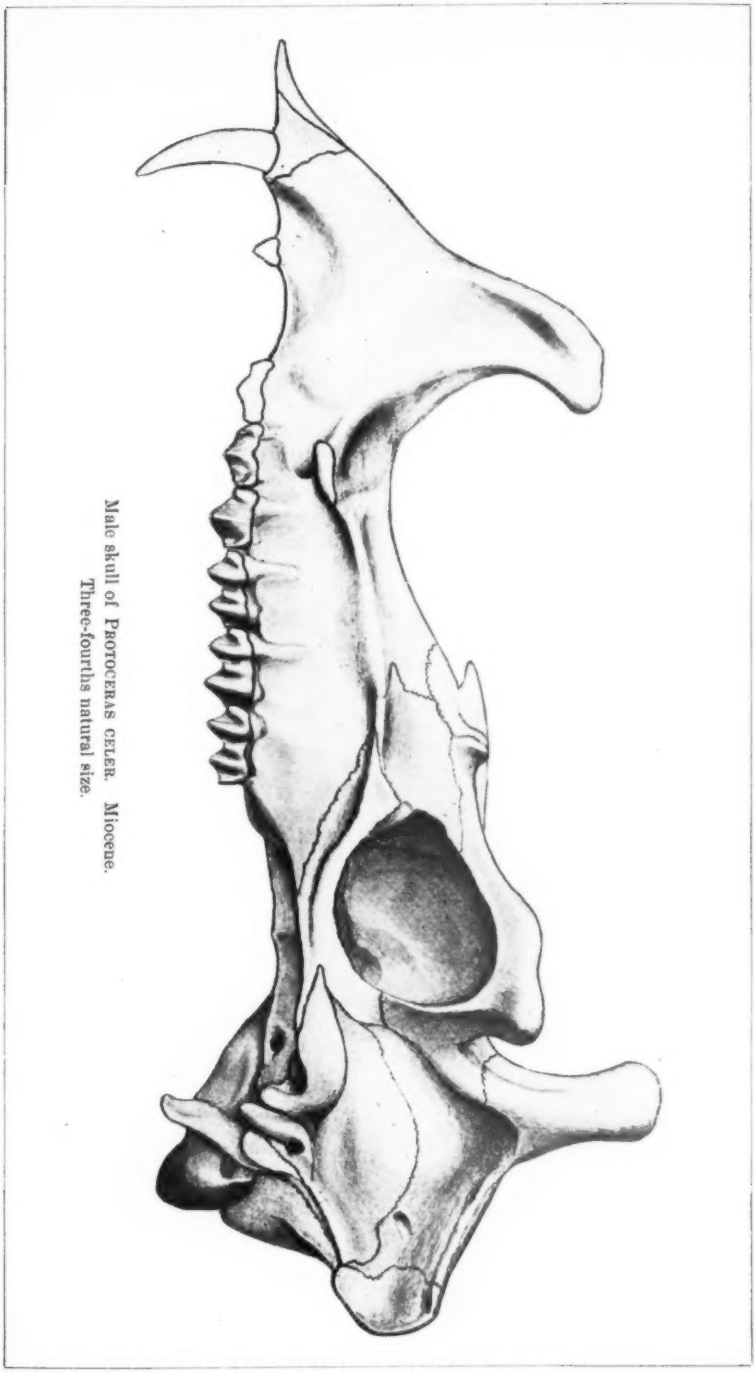
The figure will assist us in our explanation: I represents the column of air during a condensation, II during a rarefaction (fig. 2). The flame is situated at a node.

We shall suppose the maximum pressure of either sign to be 5^{cm} of water, since a singing flame does not produce a very loud note, and will take 2^{cm} as the pressure on the gas. Here is roughly what happens when the flame sings:

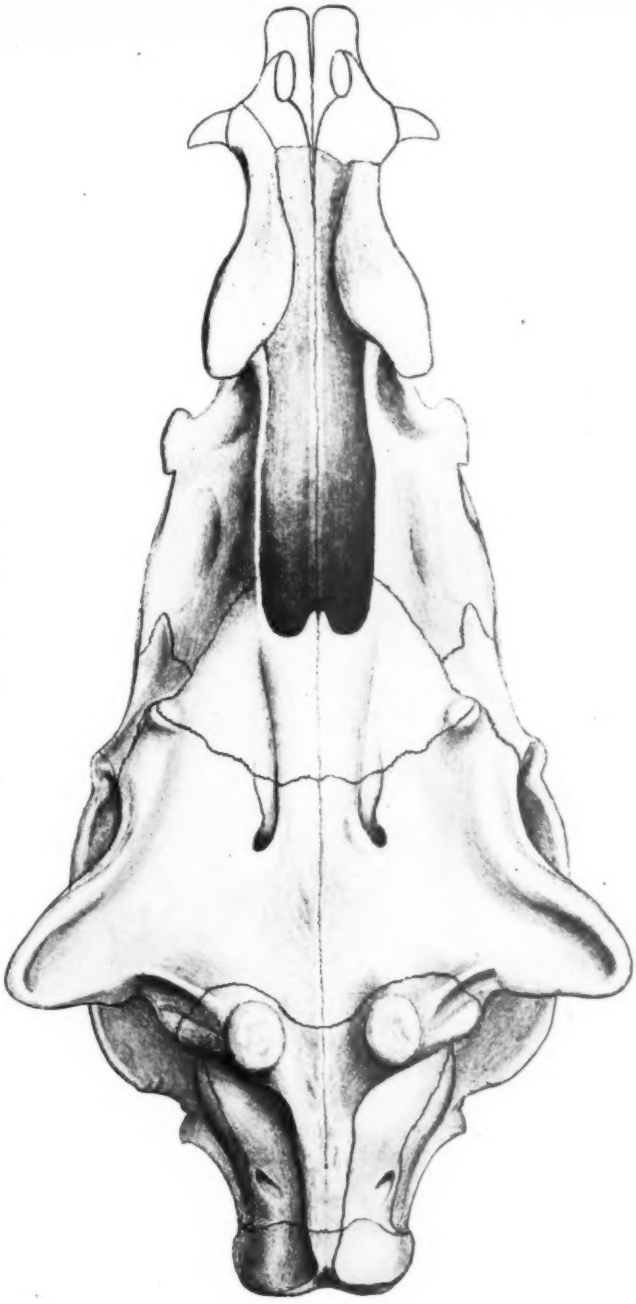
During a condensation the air is being compressed in the direction of the small arrows with a pressure represented by 5. As the burner of the singing flame is not in communication with the air in the tube except at the small aperture from which the gas issues, this pressure acts in the direction A on the gas. But the gas is issuing from this aperture under a pressure 2 in the direction G. Therefore the resultant of



Male skull of PROTOCERAS CELER, Marsh. Miocene.
Three-fourths natural size.



Male skull of PROTOCERAS CELER. Miocene.
Three-fourths natural size.



Male skull of PROTOCERAS CELER. Miocene.
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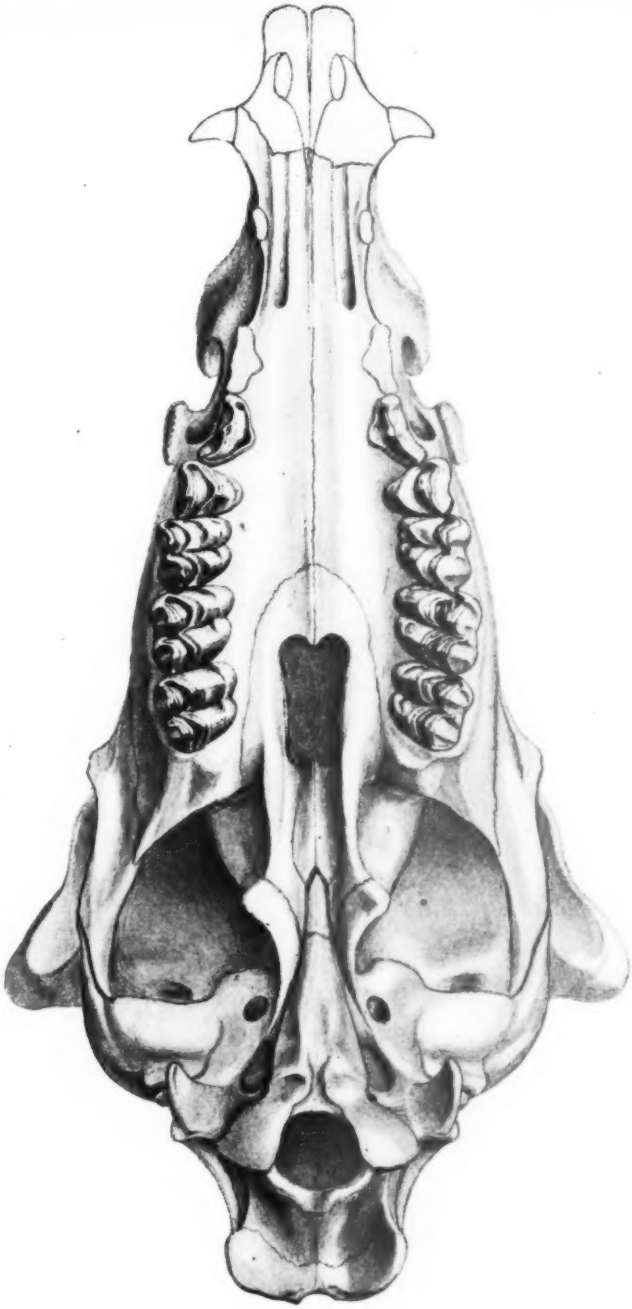

Male skull of PROTOCERAS CELER. Miocene.
Three-fourths natural size.

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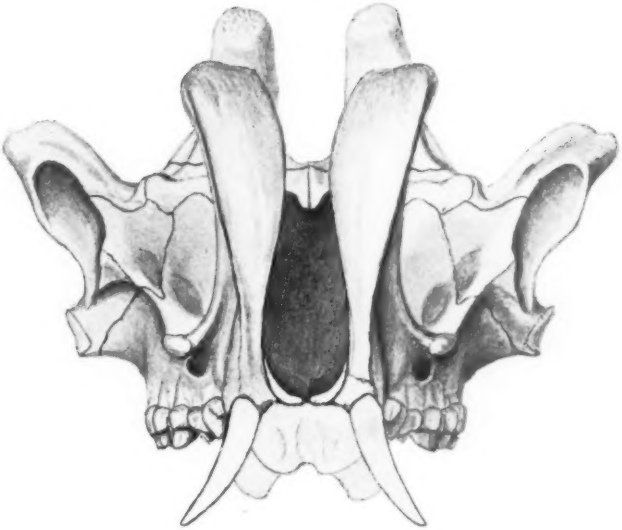

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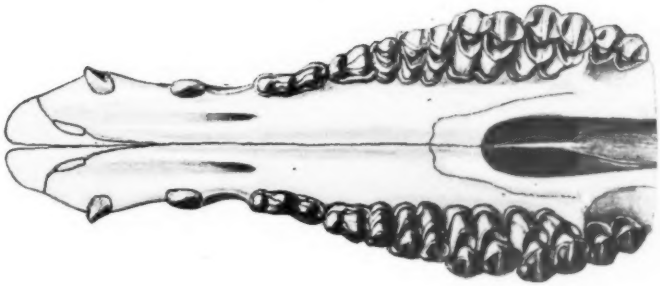
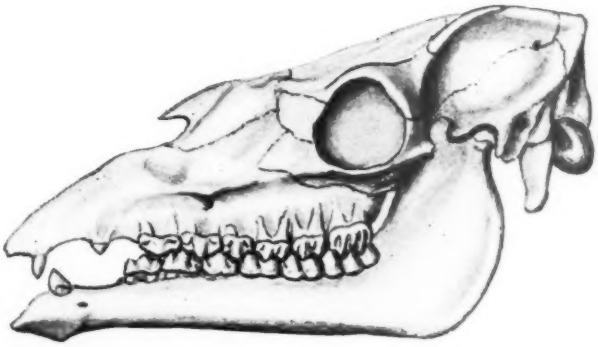


1.—Male skull of PROTOCERAS CELER. Miocene.
2.—Female skull of PROTOCERAS COMPTUS, Marsh. Miocene.

Three-fourths natural size.

1.



2.

3.

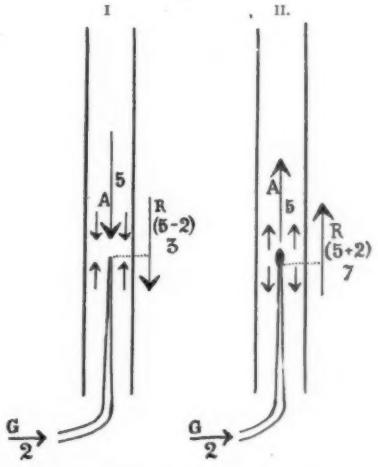
4.

1 and 2.—Female skull of CALOPS CONSORS. Marsh. Miocene.
3 and 4.—Brain cast of PROTOCERAS CELER. Miocene.

One-half natural size.

these is evidently a pressure $(5-2)=3$ acting on the gas in the direction R. This pressure forces the gas back some little distance into the gas pipe, and thus the flame is either made very small, or forced back into the burner with the gas, or extinguished altogether. But this state of things only lasts a small fraction of a second. The condensation changes into

2.



a rarefaction, and the air expands in the direction of the small arrows (II); when the pressure is taken off the gas it rushes forth. By the same reasoning as before we see the resultant pressure is $(5+2)=7$, and that the gas issues forth under this pressure. This pressure, so much greater than the normal gas pressure, causes the gas to escape with great rapidity, the flame lights up again with a slight shock (as one remarks when he lights a gas jet), this shock gives an additional impulse to the expanding air, but again comes the condensation, the flame is again extinguished and so on. Thus we see how the note is kept sounding, a very small periodic impulse being sufficient to keep a note sounding once it has begun.

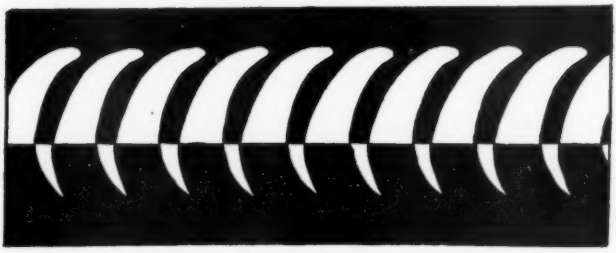
There is a point to be noted. We said the gas issued forth under a pressure of 7, but it is evident that the gas will come out as soon as the rarefaction is so far developed that the pressure of the air is a little less than 3. From this it is clear that the gas issues forth under a pressure considerably greater than its normal pressure, and that the lighting up of the flame comes at such an instant that it assists the expansion during the rarefaction.

This explanation will be found to account for all the facts which have been observed regarding the singing flame. From it we see that the chief cause to be taken into account is the pressure on the gas, although the flame also plays its part.

The following facts prove the correctness of this explanation.

We stated that the gas was forced back into the burner during a condensation, and that it may happen that the flame is forced back with it. With an aperture of such a size (0·5^{mm} in diameter) as is usually employed no such result could be observed. With a glass tube drawn to a point having an aperture of about 2^{mm} diameter, singing inside a tube 70^{cm} long by 2·5^{cm} in diameter, we noticed clearly that this result actually took place. The image of this flame as seen in the rotating mirror was like that roughly represented in fig. 3.

3.



This experiment is rather difficult to make, and the note only continues for a short time owing to the size of the aperture.

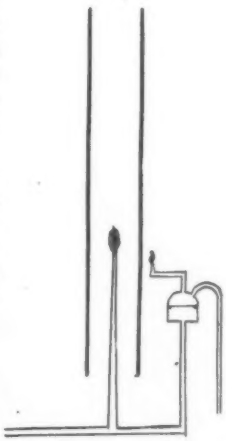
The following experiment shows the same thing in a more simple manner. As we have seen, the rapid increase of pressure during a condensation in the sounding-tube produces a downward compression on the gas. It ought, therefore, to be possible to detect this by means of a manometric flame. Fig. 4 explains itself.

When the manometric flame is observed in a rotating mirror the ordinary appearance of a Koenig flame is seen, as is represented in the lower part of fig. 5. In this figure the tongues are somewhat exaggerated in distinctness.

We can use this same arrangement for another interesting experiment. From what we have seen it is clear that the tongues of the image of the manometric flame ought to coincide with the dark spaces of the image of the singing flame, if both images could be perfectly superposed. Since the little shock on the gas is practically transmitted instantaneously throughout the

gas near the jet, the following experiment is of value. The flames, as in fig. 4, were arranged so as to be as near as possible to each other, the point of the manometric flame just reaching the base of the other. The axis of the rotating mirror was in the same plane as these two flames. On viewing the two images in the mirror, an eye placed in the plane of the flames sees the images as in fig. 5.

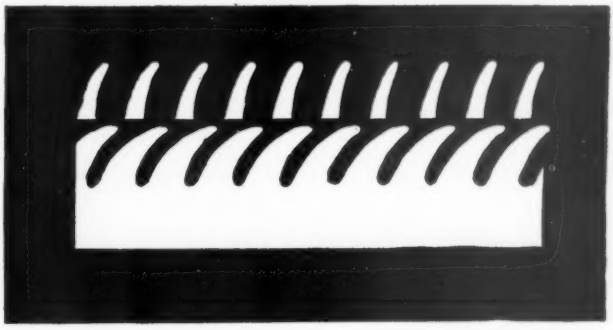
4.



The top row represents the image of the singing flame. It will be seen that what we had anticipated actually takes place. The appearance of the flame also supports our theory. When gas issues under pressure the flame drags in the air which surrounds it, thus presenting the aspect of the flame of a Bunsen burner.

The following facts show the importance of the pressure ot the gas in the phenomenon. We have seen that if the supply pipe be plugged near the jet that the flame will not sing. This is clearly because the reaction between the two pressures is interfered with. Tyndall remarks that, with a tube 15 to 20^{cm} long, he was able to obtain, by varying the size of the flame and its position in the tube,

5.



a series of notes represented by the numbers 1, 2, 3, 4, 5. He says also that this experiment shows why it happened that various experimentalists, who did not change the position and size of the flame, had difficulty in obtaining desired notes in their public lectures. We see clearly from our theory why

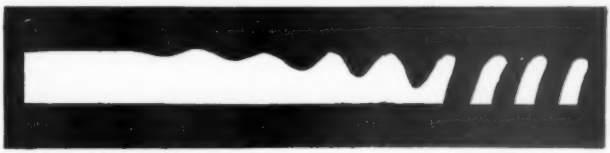
this is so, for the size of the flame depends on the pressure of the gas which feeds it. We see also why a gas-generating apparatus is disadvantageous, because, as we have before remarked, it is impossible to regulate the gas pressure with sufficient exactness. With the flask we have above described, we were able to see that the notes produced depended in great measure on the pressure, and that also the intensity of the sound depended on the same cause. We also noticed that, immediately on the note beginning to sing, the water rose a few mm. in the pressure tube, thus showing that less gas was used when the gas was singing than when silent, a fact which was also noticed by Count Schaffgotsch, from a different experiment.

Thus far we have considered a flame singing under the most favorable conditions. Often, however, if the size of the flame (i. e., the pressure of the gas) and its position in the tube be carefully regulated, the note produced, though continuous, is faint, and does not develop into a full, loud sound. The image of the flame in this case does not present a series of distinct tongues as in the former case, but resembles that of a manometric flame. On considering what we have hitherto said, this case presents no difficulty. It is only necessary to remark that the pressure of the gas and its position being such, the reactions just described are not sufficiently marked to produce the full result. If the flame be distant from the node to which its size is well adapted, it is clear, since the changes of pressure are less intense the farther we go from the node, that the flame cannot sing as it would do if under more favorable conditions. In the same way is to be explained the fact that the note is not always that which would require a node at that part of the tube where the flame is placed, for it may happen that the size of the flame corresponds better to the period of a note which has a node farther from it, which note it will reinforce rather than the one which has a node nearer to it. So also is easily explained why the flame may emit two notes simultaneously—i. e., if it be placed so as to be near both nodes. It may be said that the limit of pressure required for a given note is fairly wide.

We think we have thus sufficiently shown the importance of the pressure on the gas in maintaining the sound. We have now to show how the flame begins to sing. The flame may begin to sing without any apparent external cause, or may be put into action by an external cause. We hope to show these two cases may be reduced to the latter. Tyndall, in his usual graphic way, describes in his "Heat" how a flame, too large to sing continuously, will respond to the note proper to the tube inside which it is placed, if this note

be sounded by a siren or other means. He says that this is an example of the propagation of sound, by vibrations through the air, and its reception by a body extremely sensitive to such influences. If the flame is smaller, it continues singing once it has begun to respond. That is to say, the flame takes up the vibrations of the air inside the tube, reacts on them in the way we have described, and thus the sound is strengthened into the continuous note with which we are familiar. If we examine the flame in a rotating mirror before it has begun to sing, we see of course a continuous band of light. Now, if we sound the note of the tube, regarding the flame in the rotating mirror while doing so, we see the image as represented in fig. 6.

6.



From this we see clearly the mutual reaction between the flame and the column of air. If the flame be so small that it is just on the point of beginning to sing of itself, a sudden noise, such as clapping the hands, will cause it to begin. Always, however, the gradual development as shown in fig. 6 takes place. Again, a small flame may be made to sing by blowing gently across the top of the tube, and as before shows the gradual development. These facts are sufficient to show how the pressure due to the vibration of the column of air and the pressure of the gas react on each other. It is scarcely necessary to add that in reality this "gradual development" takes place in a very short interval of time, especially in the case of a small plane inside a short tube.

From what we have seen we are led to the conclusion that, when the flame begins spontaneously, in reality the cause is to be found in some change of pressure taking place in the column of air. If we slowly lessen the flame, before it begins to sing, regarding it as before in the rotating mirror, we notice, at a certain point, that gentle undulations appear on the border of the band of light, which follow the cause of gradual development above described. Is there any such external cause at work? We think we shall be able to show that there is. We have seen that the slight changes of pressure due to a note sounded at some distance from the tube produce changes in the pressure sufficient to cause the flame inside the tube to sing (Schaffgotsch put a small flame into action by the noise caused

by displacing a chair in the room next that in which the flame was placed). We have seen, too, that the slight changes of pressure produced by blowing across the end of the tube were sufficient, even when the flame was not small enough to begin of itself; therefore a lesser cause will suffice when the flame is still smaller. The air inside the tube is at a higher temperature than that of the atmosphere; this causes a current of air to pass upwards through the tube with a velocity which we can calculate.* This current as it passes the edges of the tube produces a faint note. The variations of pressure caused by this note are sufficient to put the flame in action. This conclusion is justified by the following facts:

(*a*) A flame begins much more easily in a long tube than in a short one, and we know the current due to the temperature of the air is proportional to the length of the tube, so that the note produced in a long tube is more intense than in a short one.

(*b*) On placing the ear near the lower extremity of a tube 60^{cm} long, inside which a silent flame burns, one hears the note produced by the current of air passing up.

(*c*) A consideration of the various ways in which a flame may be caused to sing leads to this conclusion.

Although this reaction is the chief one which causes the flame to begin to sing, we must bear in mind that there are innumerable other minor ones which, in certain cases, may play a part. Thus in the case of a very small flame inside a short tube ($8{\cdot}0$ or $10{\cdot}0^{cm}$) the expansion of the air near the flame, and accidental changes of the pressure of the gas, may be of importance.

We think we have made it clear that the pressure on the gas plays the important part in this phenomenon, and that a consideration of the reactions we have described will be found to explain the many facts noted in the case of a singing flame, some of which we have alluded to. We look therefore on the chief cause as a mutual reaction between the pressures in the tube and on the gas; the energy necessary to sustain the note being supplied by the pressure on the gas and the action of the flame. We may compare the singing flame to the siren, in which the current of air causes the disk to rotate, the note being produced by the reaction of the disk on the current of air.

* Dr. Everett investigates in his "Natural Philosophy," part II. the conditions for a good draught up a chimney and applies the formula of Torricelli for the efflux of liquids from orifices. This formula for the velocity of the air current is

$$V^2=\frac{2gha\,(t-t')}{1+\alpha t},$$

in which g=gravity; h=length of chimney (or tube); α=the coefficient of expansion for air; t=the temperature inside the chimney, and t' that of the exterior air.

II.

There is another case of singing flames which we shall briefly explain by a reasoning similar to that we have just employed. If a tube, say 30^{cm} long by 3 or 4^{cm} in diameter, be placed on a piece of wire gauze, the whole 5^{cm} above a Bunsen burner, and if the gas be lighted inside the tube, a high note of great intensity is produced. This experiment was first made by Lissajous, who called it a "whistling flame." As we have never seen any explanation offered, we think the following will be found interesting. We have determined the following facts:

1st. The note depends on the length of the tube and the volume of the flame.

2d. The gauze need not be outside the tube, but if placed inside, the note is also produced.

3d. If the gauze be more than a certain distance below the base of the tube, no note is produced.

4th. The image in a rotating mirror shows periodic disturbances at the base of the flame.

Just as a node is the position most favorable for the singing flame, a loop is that favorable for the whistling flame. There is a loop at the end of an open pipe, and hence the flame sounds when at this position. As every note which an open pipe can produce has a node at the base, we see that each can be produced by the flame at this position, and hence it is that the note is so high and shrill, for, as we shall see, a great number of tones are produced simultaneously.

At a loop, or ventral segment, there is considerable motion of the air, as is shown by placing a small tambourine with some sand on it at a loop; the sand is violently agitated by the air currents due to the vibration. The motion of the air at the extremities is sensible for some distance outside the tube. Mach studied this movement, and found that for a pipe four feet long the amplitude at this point was 4^{mm}.

We can easily calculate the velocity of these air currents if we know the amplitude of vibration and the period of the note. However in the general investigation this will not be necessary. Let us call this velocity x.

We have seen that the draught, or current due to the flame inside the tube, can be calculated from the formula

$$V^2 = \frac{2gha(t-t')}{1+at}$$

For a certain tube we have calculated that the current due to the vibrations was 2 meters per second, the draught 1 meter per second. Let us suppose the velocity of the current due to the vibration is x, that due to the draught y.

As before, let us first consider the flame actually sounding. At the node nearest the base (it is only necessary to consider one of the notes) there are alternately condensations and rarefactions. When the condensation is changing into a rarefaction there is a current of air which issues from the tube; this is the first stage. When the rarefaction is becoming a condensation this current enters the tube. During the first stage the current due to the draught tends to cross the gauze with a velocity y; but the current due to the vibration is in the opposite direction with a velocity x: therefore at this moment the resultant current has a velocity $x-y$. This current is less than if the flame had been silent, so that a smaller amount of gas enters into the flame. But after a very small fraction of a second the rarefaction at the node changes into a condensation. During this second stage the current due to the vibration goes up the tube and is in the same direction as the draught. Therefore the resultant current has a velocity $x+y$. This current, much more rapid than when the flame was silent, and than that during the first stage, causes a greater quantity of gas and air to enter into the flame than before. This sudden augmentation of the flame gives an impulse to the vibrations already taking place, and thus the note continues.

This explanation is proved by several experiments. We have stated that the gauze may be a certain distance below the base of the tube. We have seen that the vibrations of the air column extend a certain distance outside the end of the tube. Many researches have been made to determine the exact law which this distance follows, but though in individual cases it is easy to determine the amplitude, it is difficult to formulate a general law. It has been determined that this distance depends on the diameter of the tube, and that when the wave length of the note is great in comparison, this distance is somewhat greater than two-thirds the radius of the tube. We have made many experiments with the gauze in various positions, and find that the gauze must be inside the limit assigned by this law, or no note will be produced, which shows that the note depends on these air currents.

A second proof is that the note produced is not pure, but is composed of a number of different ones. We have seen that all the notes of the tube may be produced together and that thus the resultant note is composed of tones proportional to 1, 2, 3, 4, 5. . . . If the gauze be placed at the middle of the tube the note is much higher. We see at once why this must be so if the whistling flame is proper to a loop. For the only tones which have a loop at the middle of an open pipe are those proportional to 2, 4, 6, 8, etc. Therefore since so many of the lower tones are absent the resultant tone is higher than in the former case.

We have made experiments analogous to those described in the case of the singing flame to determine the manner in which the note begins. Our experiments lead to the conclusion that it is the same as in that case, and that the original note caused by the draught is strengthened by the reactions we have detailed. As the arguments used in the case of the singing flame apply with even greater force in the present case, it is not necessary to repeat them.

III.

There is, finally, one other case of a note caused by a flame which we shall consider very briefly. An experiment was suggested to test the theory in which the singing flame is explained as if it were a heated body. If so, a taper flame ought to sing if placed in a favorable position. An ordinary taper flame remains perfectly silent. On dividing up the wick so that the head of the taper stretched over the tube, a note was produced which lasted several minutes. This is merely a case of Rijke's experiment of the heated gauze above alluded to. Rijke's experiment has been explained already, and is easily understood if we consider the action of the pressure and various air currents in a sounding pipe, and bear in mind that the most favorable moment for the air to receive an increase of temperature is when a condensation is changing into a rarefaction. The following passage from a lecture by Lord Rayleigh will make this clear ("Nature," vol. xviii, p. 320, 1878): "Perhaps the easiest way to trace the mode of action is to begin with the case of a simple vibration without a steady current (i. e. the draught). Under these circumstances the whole of the air which comes in contact with the metal in the course of a complete period becomes heated; and after this state of things there is comparatively little further transfer of heat. The effect of superposing a small steady upward current is now easily recognized. At the limit of the inward motion, i. e., at the phase of greatest condensation, a small quantity of air comes in contact with the metal which has not done so before, and is accordingly cool; and the heat communicated to this quantity of air acts in the most favorable manner for the maintenance of the vibration." "Both in Rijke's and Riess'* experiments the variable transfer of heat depends on the motion of vibration, while the effect of the transfer depends upon the variation of pressure. The gauze must therefore be placed where both effects are sensible, i. e. neither near a node nor near a loop." (We have found the same in the case of the

* Riess's experiment consists of a note produced by a current of hot air passing through a cool gauze.

taper flame.) "About a quarter of the length of the tube, from the lower or upper end, as the case may be, appears to be the most favourable position." When the gauze is near the top a slight modification is required, but the theory is essentially the same.

We have, then, three kinds of singing flames; one depending on changes of pressure; another on air currents; a third depending at once on both changes of pressure and on air currents.

In the above paper we have explained the general causes of the phenomenon, but it is evident that in so complex a subject it would be impossible to enter into more explicit details in the limits of a simple article. It is hoped that these ideas will attach a further interest to this old fashioned but interesting experiment.

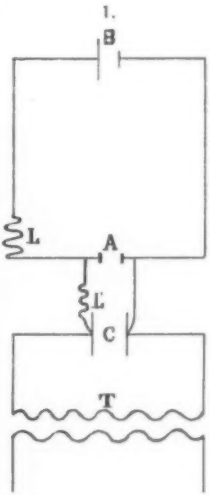
Louvain.

ART. XXI. — *Electrical Discharges in Air;* by JOHN TROWBRIDGE.

THE flaming discharge from a large accumulator with its nucleus, consisting of a dazzling white spark, is evidently a form of voltaic arc; and I was interested to discover if possible the mechanism, so to speak, of the voltaic arc. Does it follow Ohm's law in respect to resistance, and is there an oscillatory phenomenon? It is well known that electric sparks can be greatly increased in length by interposing a gas flame between the terminals of a Ruhmkorf coil, or by moderately rarifying the air between such terminals. The conditions in the voltaic arc favor a greatly increased length of a disruptive spark between the positive and negative carbons. This can be seen in the photograph of the arc produced by a high tension accumulator; and doubtless the same phenomenon could be observed in the ordinary voltaic arc if it were not so exceedingly brilliant.

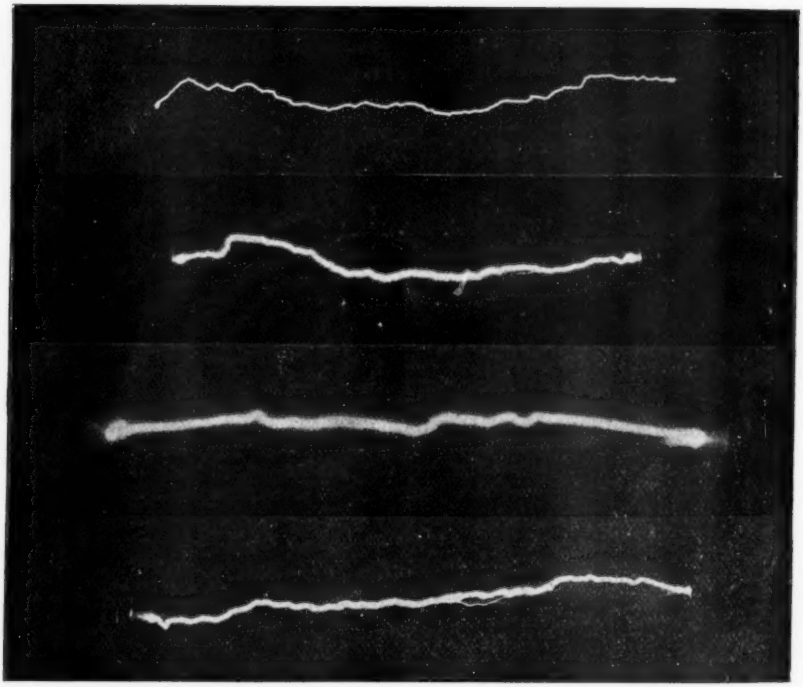
I have lately studied the apparent resistance of the voltaic arc in the following manner. In the circuit B, fig. 1, of forty large storage cells, giving 80 volts, was placed a low resistance choking coil, L, or coil of large self-induction. To the carbon terminals, A, between which the voltaic arc was produced, were led the terminals of a condenser, C. The latter was charged by a step-up transformer, T. The oscillatory discharge of the condenser was thus passed through the voltaic arc; and a spark in a gap in the circuit of the condenser was photographed by the aid of a revolving mirror. The photographs gave the

number of oscillations in the circuit containing the arc and the condenser. A curve was then plotted with the number of oscillations as ordinates and the ohmic resistance of the circuit as abscissas. It was thus found that the apparent resistance of the voltaic arc was equivalent, in the case I considered, to a resistance of eight-tenths of an ohm (·8 ohm). It was found, moreover, that an arc one-quarter of an inch long did not present more resistance than one, one-half an inch long. The apparent resistance, therefore, of the voltaic arc does not follow Ohm's law. I am led to believe that the mechanism, so to speak, of the voltaic arc is as follows: A disruptive discharge accompanies a flaming discharge, and serves as a species of pilot spark. A variable difference of potential is necessary to sustain the disruptive discharge; and this variable difference of potential makes itself evident as an apparent change of resistance; the arc shortens or lengthens in obedience to the mechanism of the lamp which is employed.



A family resemblance may be said to exist between all forms of electrical discharges in air. Thus in the voltaic arc we have a disruptive discharge combined with a flaming discharge. In general the disruptive spark is oscillatory even in the case where the voltaic arc is produced by a dynamo machine. When we extend our studies to the forms of electrical discharges which are free to a great extent from the flaming discharge, such as the disruptive sparks from electrical machines, Tesla and Thomson transformers and the Planté rheostatic machine, we are struck by their close resemblance to the ordinary forms of lightning discharge. I have lately employed, in connection with five thousand Planté cells, a Planté machine with thirty condenser plates made of glass, one-sixteenth of an inch in thickness, with a coated surface of 15×18 inches. Sparks nine to ten inches long can be very conveniently studied by means of this apparatus, for a close estimate of the difference of potential is possible and the exciting apparatus does not change its sign during the experiments. To the eye each spark seems to be surrounded by a bright radiance or aureole of which it appears to be the nucleus. In order to ascertain whether this radiance was an actual phenomenon, I employed a portrait lens of large aperture, and some of the results are exhibited in the accompanying reproductions, which fail, however, to give the details of the negatives. Fig. 2 is a

photograph of a spark taken with a Euryscope lens, such as is commonly employed for landscape work. This does not show any detail. Figs. 3, 4 and 5 are photographs taken with a Dallmeyer portrait lens, without a diaphragm, and show on the negatives what may be considered an aureole accompanying the spark its entire length. Furthermore, the oscillatory nature of the sparks is shown by forked discharges which



diverge from the main path of the spark and which point in opposite directions on the same spark. If a photograph of lightning could be obtained which would show a similar phenomenon, there could be no doubt of the oscillatory nature of lightning.

Since one can, with a large number of Planté cells in connection with a rheostatic machine, control the sign of the electric charges on the spark terminals, I was interested to test the question whether the eye can detect any direction in electric sparks. One observer, looking through an opening which concealed the spark terminals and only revealed the central portion

of the sparks, noted down his impression of the apparent direction of each spark, while another observer reversed the poles which charged the rheostatic machine. On comparing the notes of the two observers, it was found that there was no agreement in regard to direction. This result was to be expected from the oscillatory nature of the discharges. It may be that in the case of lightning the eye is forcibly impressed by the greater brightness of the positive terminal in the cloud, and the observer concludes that the flash has a unidirectional movement.

When oscillating sparks of the nature represented in figures 2, 3 and 4 are passed through Crookes tubes of the focus tube pattern, it was found that photographs could be taken on plates exposed to the inclined surface of the platinum, both when it was made the anode and when it formed the cathode. No difference in definition could be noticed. There was, however, a great difference in actinic effect. Under the oscillatory nature of the Leyden jar discharge the electrodes become alternately positive and negative. Possibly some of the want of definition noticed in Röntgen photographs, taken even with the aid of electrical machines, may be due to the fact that the oscillatory discharge does not always emanate from the same point on the anode surface. A small anode should therefore give sharper images than one of a large surface.

What is supposed to be a resistance in the case of the voltaic arc, and in the modifications of this arc seen in discharges from high tension transformers, and in powerful electric sparks, and presumably in lightning discharges, is a polarization which produces a variable difference of potential at the spark terminals.

The inconstancy of spark potentials has been shown by Jaumann.* In working with a revolving mirror, it is found that the spark terminals have to be brightened in order to preserve the same spark length. I was interested also to observe the effect of the surrounding medium upon the spark potentials. Platinum terminals in sodium vapor showed the polarizable condition: but there did not appear to be an appreciable change in resistance, apart from this polarization. The same was true when bromine vapor surrounded the spark terminals. In the case of Crookes tubes, it is customary to apply heat if the discharge will not pass through the tube: and some makers provide a connecting receptacle which contains a substance which, on being volatilized, modifies the internal conditions of the tube. This modification is often spoken of as a diminution of resistance of the tube. It should be more properly termed a method of modifying the state of polarization of the electrodes.

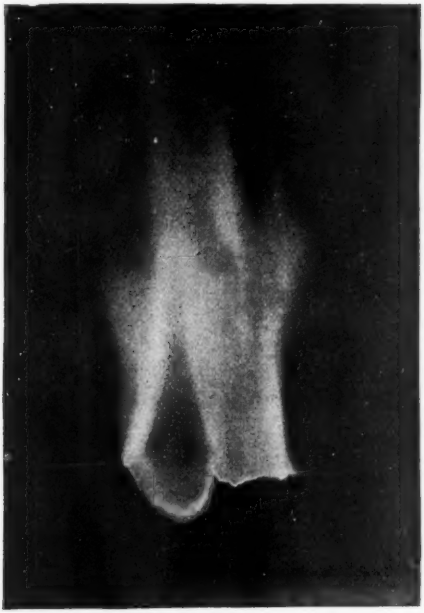
Jefferson Physical Laboratory, Harvard University.

* Wied. Annalen, lv, p. 656, 1895.

Art. XXII.—*The Oscillatory discharge of a large Accumulator;* by John Trowbridge.

The discharge from a large number of Planté cells is characterized by a sibilant flame which, by quickly separating the spark terminals, can be drawn out to a length of several feet. It closely resembles the light produced by passing an electric spark through lycopodium powder. When a photograph of

1.



this flaming discharge is examined, it is seen to have an intensely bright spark as a nucleus. On account of the flaming discharge it is difficult to examine its character by means of a revolving mirror. By employing, however, two spark gaps it seemed possible to ascertain whether the discharge is oscillatory or not.

In my experiments the circuit was made at the instant the revolving mirror was in the position to reflect an image of the discharge of the battery upon a sensitive plate. The photographs obtained in this way showed disruptive discharges superimposed upon a continuous discharge. The latter, however, masked any appearance of an oscillatory discharge. It

was evidently necessary to blow out the flaming discharge in order to see if oscillations followed the pilot discharge. The first experiment was made with 2500 cells arranged in series; and the flaming discharge was much lessened both by the reduction in the number of cells and by a suitable arrangement for blowing it out. On developing the photographs it was found that the discharge was an oscillatory one; for as many as five or six clearly defined oscillations followed the first, or pilot discharge. The number of cells was then doubled; and, although more difficulty was experienced with the flaming discharge, oscillations were again obtained.

On the supposition that each cell of the battery can be regarded as a leaking condenser: and that it is equivalent in capacity to a condenser shunted by a resistance equal to that of the electrolyte, we can treat such a cell as a conducting condenser under the influence, during discharge, of a periodic current. The analysis of this well-known case is as follows.* Let ABC and AEC be two circuits, the circuit ABC being a shunt to the circuit AEC, which contains a condenser E.

Let L be the coefficient of self-induction of ABC, R its resistance, C the capacity of the condenser in the circuit AEC and r the resistance of the wires leading to the plates of the condenser.

Then if i is the current through ABC and x the charge on the plate nearest to A

$$L\frac{di}{dt} + Ri = r\frac{dx}{dt} + \frac{x}{c}.$$

Since each of the quantities is equal to the electromotive force between A and C.

If $i = \cos \rho t$,

$$\text{then } x = \frac{(L^2\rho^2 + R^2)^{\frac{1}{2}}}{\left(\frac{1}{C^2} + r^2\rho^2\right)^{\frac{1}{2}}} \sin(\rho t + \alpha)$$

$$\text{where } \alpha = \tan^{-1}\frac{L\rho}{R} + \tan^{-1}\frac{1}{r\rho C}.$$

$$\text{Hence } \frac{dx}{dt} = \frac{\sqrt{L^2\rho^2 + R^2}}{\frac{1}{C^2\rho^2} + r^2} \cos(\rho t + \alpha).$$

Thus the maximum current along AEC is to that along ABC as

$$\sqrt{L^2\rho^2 + R^2} \text{ is to } \sqrt{\frac{1}{C^2\rho^2} + r^2}.$$

Or if we neglect the resistance r of the leading wires, as

$$\sqrt{L^2\rho^2 + R^2} : \frac{1}{C\rho}, \text{ or neglecting L, as } \frac{R}{\frac{1}{C\rho}}.$$

* See Elements of Electricity and Magnetism, Prof. J. J. Thomson, p. 431.

In the case of one cell of the battery the polarization capacity is undoubtedly very large. C. M. Gordon* finds that the polarization capacity of two surfaces of platinum $0{\cdot}65^{cm^2}$ separated by an interval of 2^{mm} amounts to more than 50 microfarads. The cells of my battery consist of lead plates of about 10^{cm^2} surface separated by about 6^{mm}. The layer of peroxide of lead undoubtedly gives a large polarization capacity. The resistance of each cell is about one quarter of an ohm. Even with this small value of R, oscillating currents such as my experiments show arise when the battery discharges through air or gases. A large portion of the oscillating currents pass through the condenser circuit, and the electrolyte acts as a semi-insulator. With a very high value of p, no current would pass through the electrolyte and the cells would therefore act like Leyden jars. In the case I am considering the Planté cells evidently act like leaky Leyden jars coupled in series. If C is the apparent capacity of one cell $\frac{C}{n}$ would be the capacity of n cells.

An examination of the photographs of the oscillations produced by 2500 cells, showed an apparent capacity of about 1000 electrostatic units. Five thousand cells gave an apparent capacity of about 500 electrostatic units, as should be the case. The small apparent capacity C results from the leaking of the condenser due to the conduction through the electrolyte.

Since the discharge from an accumulator of a large number of cells is, in general, oscillatory, I am led to the belief that the discharge from any primary battery is also oscillatory, for in all cases we have to deal with capacity and self-induction. It is evident that a galvanometer in circuit with a Geisler tube or a telephone cannot detect the oscillatory discharge, since it is of high period. Moreover when a Geisler tube is lighted by a large battery with no resistance save that of the Geisler tube and the battery in the circuit, and the light is examined in a revolving mirror by the eye, no oscillations or intermittance of light can be perceived on account of the flaming discharge through the rarified gas.

The oscillatory discharge may be said to be the common occurrence of nature in the case of electrical discharges and the one direction discharge the uncommon. This has been expressed by the remark that electricity takes the path of least resistance; this common belief, however, must be modified under certain conditions of resonance. In general nature avoids a unidirectional discharge.

Jefferson Physical Laboratory, Harvard University.

* Wied. Ann., No. 5, 1897, p. 28.

ART. XXIII.—*Jura and Neocomian of Arkansas, Kansas, Oklahoma, New Mexico and Texas;* by JULES MARCOU.

HISTORIC geology, or stratigraphic classification, is a very difficult and at the same time a most important part of the history of our globe. Without exact classification, all becomes confusion, geologic periods are confounded, and we are confronted by the same sort of errors that would occur if some historian were to place the time of Cromwell after the time of Washington.

In all other sciences, like chemistry, physics, anatomy, etc. each new fact can be verified in laboratories, after a period of time relatively short. Not that criticism, and strong and even passionate opposition, are not found in these sciences; we remember well the protest against the experiments made to disprove spontaneous generation. It required much persistency and courage on the part of Pasteur to maintain the truth he had discovered. The chemist Berthelot published in the Revue Scientifique some incomplete investigations of the late Claude Bernard, which he found in loose notes after the latter's death, without even taking the polite precaution to make known to Pasteur his intention of attacking his observations on the non-existence of spontaneous generation. Numerous discussions followed at the meetings of the Academy of Science of the French National Institute, until Pasteur, excited by the incessant attacks of two adversaries, bravely turned towards them and said to one, "Savez-vous ce qui vous manque? vous ignorez l'art d'observer;" and to the other, "Et vous, celui de raisonner."*

The controversy against my observations on the geology of Texas, the Indian Territory and New Mexico, has lasted much longer than the opposition made against Pasteur, for it is now forty-four years since I made them and they have been and are still the subject of constant criticism.

In science, discussion must be based on the observation of facts; in geology, these observations must be made on the ground and at the precise locality under discussion. For years the two localities discussed were not only far distant from civilization, but also situated in a part of the country which, on account of hostile Comanches, Kiowas and Apaches, it was impossible to visit without a large military escort. Consequently my contradictors discussed my observations without a practical knowledge of the stratigraphy; and with a want of

* Discours de M. Joseph Bertrand, directeur de l'Académie Français, séance du 28 Janvier, 1897.

the most elementary kind of knowledge of the genus *Gryphæa*, uniting into a single species six or eight entirely distinct species. It was to be hoped that when, in about 1880, Indian Territory and the Tucumcari region were finally opened for settlement and civilization, my opponents would examine the two localities; one called Comet Creek, now in G. County, Oklahoma, and the other Pyramid Mount in the Tucumcari region of New Mexico. But not at all; to this day, Comet Creek has not been visited by any other practical geologist*; and Pyramid Mount of the Tucumcari area was systematically left out of the route of exploration by the three persons who were there, since 1888. The curious part of it is that the section at Pyramid Mount is most complete, without any obscurity by vegetation, practically a bare wall, and unique in the Tucumcari region for its beauty and perfection from a geologic point of view. I shall not imitate the frankness of my friend Pasteur, and contest the capacity of my adversaries as stratigraphists and paleontologists, but I owe it to science to maintain what I consider to be exact and true; and however tired and wearied by years, by my infirmities and the exceptional length of the discussion—lasting almost half a century—I shall continue not only to affirm the correctness of my observations, but also to ask my numerous adversaries to visit Comet Creek and Pyramid Mount, and beg them to publish the sections accompanied by good figures and descriptions of all the fossils they may gather *in situ*. I am happy to remark, that they will have the great privilege and immense advantage of remaining there as long as they please, to observe and collect specimens, while I was enabled, on account of the rapidity of the march of my military escort, to remain at Comet Creek only one hour and at Pyramid Mount only three or four hours.

The question of the existence of the Jura and the Lower Cretaceous (which I call briefly Neocomian) has taken, thanks to the opposition, such great proportions, that one of my opponents said lately: "There are reasons for suspecting that no marine Jurassic formations of Atlantic sedimentation have as yet been discovered north of Argentina (South America) on the present Atlantic slope of the American hemisphere." (Science, vol. iv, No. 103, p. 920.) A clean sweep of the marine American Jura.

Let us review the main localities in the United States, west of the Mississippi River and east of the Rio Grande del Norte.

Arkansas.—For the sake of brevity, and not to burden the

* Lately the locality of Comet Creek has been visited by Mr T. W. Vaughan, who finds the same beds of limestone containing *G. Rœmeri* (called *G. fornicula*ta). His description does not differ from the one I have given as far back as 1853. "Outlying areas of the Comanche series"; this Journal, vol. iv, July, 1897.)

reader with too many details, I shall speak only of the Trinity formation, of the locality in Pike County, Arkansas, close to the boundary line of Texas. Mr. Hill, the inventor of the name Trinity formation, has published in the Annual Report of the Geological Survey of Arkansas, for 1888, vol. ii, Mesozoic, two chapters, xii and xiii, in which are described the strata and the fossils, the latter with figures. It is useless to reprint what I have said on each species of fossil; I need only say, that I have shown with accuracy and details, in the American Geologist, Dec., 1889, pp. 357–367, that the whole fauna, without a single exception, is composed of Jurassic fossils, and concluded that instead of being Lower Cretaceous, the strata near Murfreesboro represent in Arkansas and Texas the superior Jura from the Oxfordian upward, including the Purbeck formation.

As an example of carelessness, not to use a stronger word, in quoting a plain paleontological fact, I call the attention of the reader to a quotation of Mr. Hill, at p. 128. In the description of *Ammonites Walcottii*, we read: "It resembles . . . also *Ammonites Yo*, d'Orb. of the Lower Cretaceous." Turning to the great work of the *Paléontologie Française* by d'Orbigny, in order to examine and verify the resemblance of the two Ammonites, I naturally took up the volumes entitled: "Terrain Crétacé." But there is no trace of *Ammonites Yo* in those volumes. As I know the species well and that the form is undoubtedly Jurassic, I took the volumes entitled: "Terrain Jurassique," and there in vol. i, pp. 545–546, is the *Ammonites Yo* with the special location of "Etage Kimmeridgien, Boulogne-sur-Mer." Instead of belonging to the Lower Cretaceous, according to the extraordinary alteration of Mr. Hill, it is an Upper Jura species. Why Mr. Hill took upon himself to change the age of the *Ammonites Yo*, cannot be explained otherwise, than that he wanted to sustain his classification of the Trinity division in the Cretaceous, quoting in his favour the great authority of d'Orbigny. This case shows how unreliable Mr. Hill is, when he writes on paleontology.

Kansas.—As long ago as 1888, I corresponded with Professor F. W. Cragin of Topeka, in regard to a *Cycadoidea* found in Maryland, for the purpose of comparing it with a specimen of the same genus of fossil plant collected in Kansas. On reading Professor Cragin's first two papers on the geology of a part of southern Kansas comprising Barber, Pratt, Kiowa and Comanche counties, south of the Arkansas River, in the upper region of Medicine Lodge River, I thought that the Jura existed there, and wrote so to him. He sent me a small box of specimens, all Cretaceous fossils. After an exchange of a few letters on the subject, the correspondence was dropped.

Lately there came into my hands two publications on these Kansas counties. One is a detailed description of them by Professor Charles S. Prosser, in vol. ii of The University Geological Survey of Kansas, Topeka, 1896; and the other is a paper entitled: On Outlying Areas of the Comanche Series in Kansas, Oklahoma and New Mexico, by Mr. R. T. Hill, this Journal, vol. l, pp. 205–234, issued in September, 1895, but which entirely escaped my notice until one week ago, on account of my time having been completely occupied for several years by the writing and printing of the Life of Louis Agassiz.

I have studied with interest all the part of vol. ii, Kansas Survey, entitled "Cretaceous-Comanche series of Kansas," pp. 96–181. The author gives carefully observed sections, and an important geological map of "Southwest Comanche area." He is clear and exact, but the paleontological part is not only very meagre, but also incorrect so far as relates to the principal and very important fossil found, a rather large *Gryphæa*, collected on the top of one of the Belvidere sections. As he follows and uses the classification of Mr. Hill, my answer will apply to both memoirs.

According to two lists of fossils determined by Mr. T. W. Stanton, at pp. 216 and 219 of Mr. Hill's paper, the *Gryphæa Tucumcarii* Marcou has been found at Blue Cut Mound, four miles southwest of Belvidere, above the *Gryphæa forniculata;* and Mr. Stanton adds: "It is interesting to note that this form (*G. Tucumcarii*), supposed by Prof. Marcou to be Jurassic, here occurs *above G. forniculata*, which he considered Neocomian, though there is only a few feet difference in the beds and they seem to be connected by intermediate forms. The geographic distribution of the two species is about the same" (Hill's paper, p. 216).

At the beginning of the controversy by Mr. James Hall in 1855, continued afterward by Dr. B. F. Shumard, Dr. J. S. Newberry, Mr. R. T. Hill, and others too numerous to name, I realized that a misuse of paleontology had been made, and ever since paleontological misrule has held complete and unchecked sway in regard to the numerous *Gryphæa* found in the whole region south of the Arkansas river. After receiving specimens now and then from Texas and Kansas, I saw clearly that about eight or ten *Gryphæa* existed there at different levels, and that the confusion of species by Messrs. Hall, Rœmer, Shumard, Gabb, Charles A. White, Hill, Cragin and Stanton, was sure to result in a complete revision and exact description of all the *Gryphæa* found in the region. I know, and I have repeatedly said, that the identification of the numerous *Gryphæa Pitcheri* was wrong in almost every case, my own

included. Professor Ferdinand Rœmer made the mistake in 1849 and 1852 in referring a New Braunsfelds and Red River species of *Gryphæa* to the *G. Pitcheri* of Dr. Morton. Following Rœmer, and on account of a complete lack of specimens for comparison, I referred the *Gryphæa* of Comet Creek to the *G. Pitcheri*, although I was in doubt as to the correctness of the determination, for my specimens differed considerably from those figured by Rœmer, and from the one figured by Morton. But at the same time I took the precaution to publish excellent and exact figures, in my two works, issued in 1855 and 1858. For several years, after my hasty visit to Comet Creek, I was convinced that the *G. Pitcheri* found there was a different species from the one published by Rœmer and the one published by Morton; and in 1861, I called the Comet Creek species *G. Rœmeri*, and have even since used that very appropriate name for the *G. Pitcheri* published with figures by Rœmer and myself. ("Notes on the Cretaceous of Texas." Proc. Boston Soc. Nat. Hist., Jan., 1861, vol. viii, p. 95.) Dr. C. A. White did not make use of the name *G. Rœmeri* and without explanation he many years after called the species *Exogyra forniculata*, changing the generic and the specific name.*

This is the way that erroneous paleontology has been constantly used by my adversaries.

We read in Mr. Hill's paper, pp. 225–226: "The species called throughout this paper *Gryphæa forniculata* White, is the same as the one from Comet Creek, Oklahoma, first figured by Prof. Marcou as *Gryphæa Pitcheri* Morton, and later called by him *Gryphæa Rœmeri*. The nomenclature of the Gryphæata oysters of the Comanche series will be thoroughly revised in a separate paper which the writer has in print. Prof. Marcou's name *G. Rœmeri* probably has precedence over *G. forniculata* White, but it may be shown neither of these will stand."

"This Gryphæa so abundant at Belvidere is likewise found in great numbers in the Kiamitia clays, not only about Denison and Fort Worth, but also along a persistent line of 300 miles from Goodland, Indian Territory, to south of the Brazos in Texas. Its hemera (*sic*) in Texas is exclusively confined to the Preston beds, and Prof. Marcou has always held that it is a Cretaceous form; it is the species upon which he established the existence of the alleged Neocomian in America."

* The *Exogyra Texana* Rœm. or *Exogyra flabellata* Goldf. is also a sort of polymorph fossil like the so-called *Gryphæa Pitcheri*. The only way to put an end to the confusion created by calling almost every Exogyra, *Ex. Texana* and every Gryphæa, *Gr. Pitcheri*, is to make a complete revision of all the Exogyra and Gryphæa existing, with very careful study of each species and the exact location of each stratum.

"An interesting fact in the Black Hills and Blue Cut sections is that the large Gryphæa which comes in near the top of the shales is identical with the form collected by Prof. Marcou and is the species called *Gryphæa Tucumcarii* by him (later called *Gryphæa dilatata* var. *Tucumcarii*)."

"Prof. Marcou insisted that the beds from which this species came were of Jurassic age, and upon its occurrence he maintained the existence of the Jurassic system in this region. It occurs at Belvidere, as on the original plains of the Kiamitia near Goodland, in Indian Territory, where it was last year collected by Mr. T. Wayland Vaughan* of my division, and at Kent† in Trans Pecos, Texas, stratigraphically above and intimately associated with the species which he calls *Gryphæa Pitcheri*. Thus we have in Kansas and Indian Territory Prof. Marcou's alleged Jurassic species occurring stratigraphically above species he called Cretaceous, which facts forever remove any previous doubt, if any existed, in favor of his theory of the existence of the Jurassic formation in Texas, Indian Territory, New Mexican region."

There is only a little difficulty in accepting the conclusion drawn by Mr. Hill with such confidence,—the *Gryphæa* collected in great numbers at the top of the section of Blue Cut Mound, near Belvidere, is not the *Gryphæa Tucumcarii!*

I have had in my possession, ever since 1888, beautiful and perfect specimens of that *Gryphæa*, and the idea that a paleontologist of the U. S. Geological Survey and a chief geologist of that Survey should call it *G. Tucumcarii!* was far from my thoughts. When such discoveries were made, as those claimed by Messrs. Hill, Stanton and Vaughan, their first duty was to give good figures and exact descriptions of the *Gryphæa* and put it side by side with the figures and descriptions given by me in 1858 (Geology of North America, etc., plate IV and pages 43 and 38, Zurich, 1858). But as it is a simple assertion, presenting no basis for discussion, the reader of Mr. Hill's paper cannot judge from the paper itself. The *Gryphæa* found on the top of the Belvidere section, above the beds con-

* In Science, April 2, 1897, vol. v. No. 118, p. 559, Mr. T. W. Vaughan says that he found in the vicinity of Arapaho (Oklahoma and Indian Territory) the *Gryphæa Tucumcarii* of Marcou, a fossil asserted by him to be Jurassic, which often occurs imbedded in the same matrix (as the *Gryphæa Pitcheri* of Marcou or *forniculata* of White). Thus he extends the error of Messrs. Hill and Stanton farther south than Belvidere (Kansas). Figures and description of the so-called *G. Tucumcarii* are entirely wanting, and Mr. Vaughan merely makes an assertion without paleontological proofs.—Note by J. M.

† At Kent the *Gryphæa Tucumcarii*, which there is the true species, is not *above* the *Gryphæa Pitcheri* (*G. Rœmeri*) but *below*. And the pretended *G. Pitcheri* of Messrs. Dumble and Cummins belongs to two new species entirely distinct from the *G. Rœmeri* or *Pitcheri* (see "The Jura of Texas," loc. cit., p. 153).—Note by J. M.

taining the *Gryphæa Rœmeri* of the Neocomian of Comet Creek, is an entirely different species from the *G. Tucumcarii*, it is a new species which I propose to call *Gryphæa Kansana*. It possesses all the main characters common to all the *Gryphæa* of the Neocomian or Lower Cretaceous of America and Europe. As for Mr. Hill's announcement that "The nomenclature of the Gryphæta oysters of the Comanche series will be thoroughly revised in a separate paper which the writer has in print," almost two years have passed and the paper is not yet distributed. I hope that it will soon be out; and then, I shall publish figures and description of the large *Gryphæa Kansana*, n. sp. Messrs. Hill and Stanton do not seem to realize that if their identification of the Gryphæas at Belvidere and at the Tucumcarii region were correct, their discovery of one above another at one place and the reverse at the other place, in the same geological basin, among almost horizontal strata, would go far towards destroying the paleontological character for classification of strata, discovered almost one century ago, by William Smith, the author of "Strata identified by Organized Fossils" (4to, London, 1816). Happily their identification of the *Gryphæa Tucumcarii* with the *G. Kansana* is incorrect and their classification is based on paleontological misrule. There seems to be a sort of fatality, after the numerous false identification of half a dozen and probably more different species of *Gryphæa*, with the *G. Pitcheri*, to have now the same difficulty of incorrect identification of two distinct species of *Gryphæa*. It is discouraging in the extreme to see such a succession of blunders during more than forty years.

Now a few words on the age and classification of the Belvidere section and other outcrops in Kansas.

Professor Chas. S. Prosser gives a very good account of the strata under consideration, in vol. ii of the Kansas Geological Survey, although he also falls into the error of calling the new *Gryphæa Kansana*, of the top of the Belvidere section, *Gryphæa Tucumcarii*. Above the New Red sandstone formation of the plains south of the Arkansas River, lies in discordance of stratification a sandstone, called by Professor Cragin "the Cheyenne sandstone," of a thickness of about 50 to 60 feet. No characteristic fossils have yet been found in it. In fact fossils are very rare, only a few shells referred with doubt to *Avicula* and *Cucullea* having been found, and these so poorly preserved that Mr. Stanton has declined "to identify them even generically." In the upper part of the Cheyenne sandstone, a small flora, eight species, has been determined by Professor Knowlton, who says: "That up to the present time the dicotyledons from the Cheyenne sandstone are not known outside of the Dakota formation," that is to say the base of the

Upper Cretaceous or Cenomanian of Europe. No conclusion can be drawn from such a meagre florula. A Cycadæ, called *Cycladoidea munita* by Cragin, recalls the Cycadæ of the Purbeck beds of the island of Portland in England. As to the impossibility of having dicotyledonous plants in the Jura, as has been insisted upon by my adversaries, it is a very hazardous supposition without any solid basis to rest upon. The great number of species of dicotyledonous plants of the rich flora of the Dakota formation, indicates that we must expect to find dicotyledonous plants far below that formation; and to say that dicotyledonous plants did not exist during the Jurassic period, is merely a supposition, based on negative proof; a very uncertain, questionable basis to rest upon in our time of belief in evolution as well of plants as of animals. After the ill success of the great paleobotanist Oswald Heer, in the use of negative proof, to deny the existence of dicotyledonous plants in the Cretaceous of America, it is rather strange to see paleobotanists in America falling into the same error.

As the Cheyenne sandstone does not exist everywhere in Kansas, where the Neocomian, called Kiowa shales, is seen, as in Central Kansas, McPherton and Saline Counties; and as at Comet Creek, the Neocomian, with *G. Rœmeri*, rests in such places directly on the New Red sandstone rocks, it is most probable that it belongs to the Jurassic period, and is an eastern prolongation of the yellow and white sandstone of Pyramid Mount. It has the same lithology with brilliant colors, as noted by Professor O. C. Marsh, and future researches will decide its real and exact geological age. A great *desideratum* is the careful examination near Belvidere of the contact of the Cheyenne sandstone with the first three or four beds resting on it. Some sort of discordance, due to erosion and denudation, and perhaps also some little difference in the dip of the strata—a difference which can be only very small considering the almost horizontality of the strata of the plains—may be detected. Of course it will require prolonged research and a very acute practical geological mind, to discover such discordance. But I hope that some day the work will be undertaken. For me such a discovery is only a question of time. So far the discordance may be looked for between No. 6 and No. 7 of Mr. Hill's section. The Kiowa shales of Professor Cragin, so well described by Messrs. Cragin and Prosser, represent the Neocomian or Lower Cretaceous in Kansas, from No. 7 of the Belvidere section up; below they may be Jurassic.

Oklahoma.—The Comet Creek bed, as it is called by Mr. Hill, is not composed "of a single stratum of Gryphæa limestone, five feet thick," as he says (this Journal, vol. l, p. 228); but of five strata or beds, which are described in my "Field

notes," published in vol. iii, Pacific Railroad Explorations, p. 131. Another example of want of exactness in quotation in Mr. Hill.*

The Tucumcari region.—It seems superfluous for me to speak again of my researches; but Mr. Hill's paper obliges me to state more forcibly, if possible, all the facts on which I have founded my conclusions that the Jura exists in this region.

The only section I was able to see and make during my very short stay of only 24 hours, the 21st and 22d of Sept., 1853, owing to the rapidity of our military march, was at an isolated peak west of the Big Tucumcari Mount, which I have called Pyramid Mount. Since the name has been used in the map of Lieutenant A. W. Whipple's expedition, and on my geological map of New Mexico, its geographical position is well established. I chose that hill on account of its complete isolation, and also because, after looking carefully through a spy-glass at the whole surroundings of Plaza Larga, I thought that the beds seemed better exposed to view and would afford me a good section. In this I was not disappointed, for as soon as I reached the foot of the hill, I had before me a most perfect geological section, almost like a wall, with every bed finely in view and accessible. I took the right side of the wall section, and carefully noted every thing I saw. As I have repeatedly published this section, I shall not give it again, only I would say that I did not see any mark of discordance of stratification by break or erosion between the beds of the New Red sandstone and the Jurassic formation. The bed of blue clay, above the yellow and white sandstone, containing near its base the *Gryphæa Tucumcarii* and *Ostrea Marshii*, is fully in view. Before reaching the bed of *Gryphæa*, I failed to find in the sandstone a single fossil. Above the blue clay with *Gryphæa Tucumcarii*, 30 feet thick, there is 52 feet of yellowish and white limestone. In this limestone a few *G. Tucumcarii* were seen, also one or two not well preserved shells of lamellibranchiæ.

I nowhere found the *Ammonites* (*Schlœnbachia*) *Shumardi*, even the smallest fragment. And from the nature of the rubbish (débris) at the foot of the wall of that section, which I examined with great care, I can say, that it is my conviction

* To finish with misquotations, I give the following foot note of Mr. Hill's paper, entitled: "A question of classification" (Science, vol iv, No. 103, p. 918, Dec. 18, 1896): "With the exception of Prof. Jules Marcou, who originally maintained that the Middle and Lower Cretaceous of Texas and the Plain Tertiary were Jurassic, and who still maintains the Jurassic age of the Middle Cretaceous beds of New Mexico and the Lower Cretaceous of Texas. This position has been disproved by research." It is sufficient for me to give it without comment, for seldom has an adversary been cited so inexactly or his researches so incorrectly used.

that the *A. Shumardi* does not exist anywhere at Pyramid Mount; therefore its stratigraphic position in the Tucumcari region must be above the last bed of white limestone, seen at the top of Pyramid Mount. This remark is of great importance, for Professor A. Hyatt, in his exploration of another part of the Tucumcari region, insists that he has found the *A. Shumardi* in company with *Gryphæa Tucumcarii*. However great may be my respect and sympathy for Professor Hyatt, and though I fully acknowledge his great authority on cephalopods, regarding him as one of the few masters on everything touching *Ammonites*, *Nautilus*, *Lituites*, etc., I cannot refrain from expressing my doubt in regard to his finding *A. Shumardi* is the same layer, side by side with *G. Tucumcarii*. That Professor Hyatt saw the Ammonites very near the *Gryphæa*, I have no doubt; but I am sure the Ammonite must be above, even if only a few inches separate them. If the *G. Tucumcarii* exists above the *A. Shumardi*, and this is a well established fact, which can be easily proved by a careful and exact stratigraphist; then *A. Shumardi*, as I have said, is a Jurassic species, notwithstanding that it belongs to the genus *Schlœnbachia*, which in that case made its appearance in America earlier than in Europe.*

That beds found at other parts of the Tucumcari region, lying above the two feet of white limestone of the top of Pyramid Mount, belong to the Lower Cretaceous or Neocomian is very probable, but I am not to be criticised for not finding them, for they do not occur at my section of Pyramid Mount. I found there only the Jura and I followed all the rules of stratigraphic classification, in referring the strata of Pyramid Mount to the American Jurassic formation. I saw clearly when I was on the top of Pyramid Mount, that on the mesa or plateau of the Llano Estacado, more especially toward the east, at Monte Revuelto, there was another series of strata capping the Jura of Pyramid Mount and consequently

* In my paper, "The Jura of Texas" (Proceed. Boston Soc. Nat. Hist., vol. 27, p. 155), I say that Professor A. Hyatt has found at the Tucumcari region the *Ammonites* (*Schlœnbachia*) *Shumardi* with the *Gryphæa Tucumcarii*, "and there is therefore no doubt possible in regard to the contemporaneity of the *A. Shumardi* and the *G. Tucumcarii*." Messrs. Dumble and Cummins in their Kent section (American Geologist, vol. xii, 1893, pp. 309-314), also regard the *A. Shumardi* as contemporary with the *G. Tucumcarii*. Although I was not convinced by the assertion of Messrs. Hyatt, Dumble and Cummins, I admitted it, leaving the responsibility on those observers. But now, after the repeated failure of my adversaries to give the exact position in the strata of each species, I am unwilling to accept any longer such supposed contemporaneity, and I think that *Ammonites Shumardi* is younger than *Gryphæa Tucumcarii* and is placed above it in the strata; and that the line of separation between the Jura and the Neocomian at the Tucumcari region and at Kent, is between the beds containing the *G. Tucumcarii* and those above containing the *A. Shumardi*, with a break or discordance of some sort between them.

younger; but it was impossible for me to go to Monte Revuelto, or any other part of the Tucumcari region, to explore that upper series, and consequently I cannot be responsible for not having found at the Tucumcari region the Lower Cretaceous or Neocomian. It is very unjust to attack me in regard to the age of strata which I have not seen, and above all it is highly erroneous to deny the existence of the Jura at the Tucumcari region. My adversaries have made use of the anti-stratigraphic method of placing in a single list all the fossils they got there, without any regard to their relative positions in the strata. This placing in a single mass all the fossils of the Tucumcari region is not the way to make exact observations. I protest loudly against such a procedure. For it is easy to see the special purpose in view, viz: to show Cretaceous species mixed with Jurassic ones, and to conclude that the Jura did not exist there. It is contrary to careful stratigraphy to do so, and very unfair. Each group and even each bed should be given with the list of fossils found in each, otherwise lists of fossils all together are deceptive, and cannot be accepted as paleontological proof of the age of all the strata of a whole region. That the Lower Cretaceous exists at the Tucumcari region I am ready to believe, but the beds belonging to it are all above those which I have classified as Jurassic. In conclusion, I have to say that in the paper of Mr. Hill we have another example of erroneous determination of a *Gryphæa* which has led some geologists to continue their disbelief in the existence of the Jura at the Tucumcari. The Tucumcari region is open to every observer, and I am fully confident of the final verdict.

Tabular view of the strata of the Tucumcari region.

Cretaceous.	Cretaceous strata of Monte Revuelto, six miles east of Pyramid Mount. Thickness about 200 feet. The contact bed with the Jurassic formation has not yet been described.
	Nota bene.—Very likely a discordance of some sort exists at the contact of the Jura and Cretaceous.
Jura.	Jurassic strata existing above the top bed of the Pyramid Mount section. Thickness unknown.
	Jura of Pyramid Mount, with *Gryphæa dilatata* var. *Tucumcarii*, and *Ostrea Marshii*. Composed of yellow and white sandstone, of blue marl, and yellow and white limestone. Thickness about 200 feet.

Keuper or variegated marls of the Trias.

General Classification of the Jura and Neocomian south of the Arkansas river.—When, in 1887, Mr. Hill published his first essay of a "Geologic Section of the Cretaceous strata of the State of Texas, etc." (this Journal, vol. xxxiii, p. 299, April 1887), I saw that there were important mistakes in his classification of the Lower Cretaceous; but I thought that he would correct them, in his further researches. Instead of improving his classification, however, and giving a clearer and more exact nomenclature, confusion has been aggravated, until I think it is time to interfere.

It is strange, that almost perfect stranger as I am, for I have explored a very small part of Texas, only a simple road in the Panhandle—I should be obliged again to attempt a more logical and exact classification, than the one in use by those who have in charge the description of the geology of that great and beautiful State.

In 1860, Dr. B. F. Shumard published his "Section of the Cretaceous strata in Texas" (Trans. Acad. Sc. of St. Louis, vol. i, p. 582); and in 1861, I gave before the Boston Society of Natural History a corrected section, reversing the whole classification from top to bottom (Proceed. Boston Soc. Nat. Hist., vol. viii, January 1861, p. 93). After many years of opposition and discussion my classification has been accepted as correct. It remained only to complete the details of subdivisions, and to keep the Jurassic formation distinct from the Lower Cretaceous. Mr. Hill in his first paper replaced the Jura in the Cretaceous, and ever since with some variations has continued to identify the Jura either with the Washita division, or with the lower part of what he calls the Lower Cretaceous (Trinity division). Only once, after his first visit to the Tucumcari region, has he recognized the strata containing the *Gryphæa dilatata* var. *Tucumcarii* and *Ostrea Marshii* as Jurassic; but as he said, he regretted it directly, and returned with new vigor to his classification of the Texas Jura, in the Lower Cretaceous.

The constant changes in Mr. Hill's classifications render it extremely difficult to understand the meaning of the names he uses for the subdivisions, the great divisions and even his name of "Comanche series," called first "Texas series."* It is almost impossible to keep pace with him. Almost every year since 1887, when he published his first classification, he has brought forward a new one. To render the matter more confusing, the Geological Survey of Texas has also used different classifications in its annual reports, and we have now about ten different classifications to deal with.

*Mr. Hill, in his nomenclature, uses the name "Comanche Peak Group," "Comanche Division" and "Comanche series;" rather too many Comanches.

For the present I shall not enter into any explanation of these classifications or show the numerous discrepancies between them. But I shall say only, that above what has been called "Dinosaur sand" or the true Trinity, we have a certain number of beds, called Glen Rose, Paluxy, Caprina and Caprotina limestone, Gryphæa rock and Walnut clays, Comanche Peak chalk and Goodland limestone—some of which are placed sometimes in the Trinity division, and at other times in the Fredericksburg—which require a careful study before they are placed either with the "Dinosaur sand" or with the Washita division. With this reservation I shall proceed to present a stratigraphic and paleontologic classification of the strata comprised between the undoubted Trias and the undoubted Upper Cretaceous.

A. Beginning with the base, we have first, the Trinity or Bosque division. The first description of it was given by Mr. Hill, in the second volume of the Arkansas Geological Survey, 1888. I repeat that I have shown with details, that the fauna found in it is a Jurassic fauna, without a single form which can be attributed to a Cretaceous species. In Pike County, Arkansas, round Murfreesboro, the upper portion of the Trinity division has been destroyed by denudation and erosion after the upheaval and break at the end of the Jura period. But it may turn out after more minute researches and observations, that the missing upper portion may be found in part, if not *in toto*, somewhere between Murfreesboro, Pike Co., Arkansas, and Tishomingo, Chickasaw Nation, in the Choctaw Nation territory, at or near the small stream called Kiamishi or Kiamashi, where Dr. Z. Pitcher found a small specimen of a *Gryphæa*, called by Dr. Morton *Gryphæa Pitcheri*.

The geographical distribution of the Trinity division follows a curved line from Murfreesboro, Arkansas, to Tishomingo, to Glen Rose, Somerville Co., to Bosque Co., and Trevis Co. in Texas, crossing the Trinity River valley, from which it received its first name from Mr. Hill. Two different *facies* exist on that line. The first or Arkansas *facies* is composed of various rocks, such as grayish yellow limestone and argillaceous sand. The second or Bosque *facies* is less argillaceous, with more limestone, more especially in that portion of the division which has been called "Glen Rose (alternating) bed."* West of Travis Co. or Austin, a third *facies* makes its appearance, formed only of sandstone yellow and white,

* The superposition of some of the subdivisions referred to the Glen Rose is doubtful, and some may belong to a younger formation. The Paluxy sand has also been classified by some as belonging to the Fredericksburg division, while others place it with the Trinity sand, without intercalation of the Glen Rose.

which has been called Paluxy sandstone near Comanche Peak and at the Kent County section; simply yellow and white sandstone at the Pyramid Mount section; and Cheyenne sandstone near Belvidere, Kansas. This third *facies* is the most persistent, occupying all the western country from Kansas to New Mexico, western Texas and very likely northern Mexico.

B. Following the Trinity division and in concordance of stratification we have the Tucumcari division, composed of blue marl at the base, containing a bed formed entirely of *Gryphæa dilatata* var. *Tucumcarii* and *Ostrea Marshii*; then comes a yellowish limestone with two or three feet of white limestone. The top of that formation has not yet been described. Very likely it exists at Monte Revuelto, in the Tucumcari region. The *Gryphæa Tucumcarii* is found scattered, more or less, through the whole division, but more abundantly at the base. At the end of the deposition of this formation, a great break occurred; denudation on a large scale swept away the greatest part of it, leaving now and then a small remnant; for instance at Belvidere, Kansas, where the strata numbered 4, 5 and 6, by Mr. Hill seem to belong to the lowest bed of the blue clay containing *Gryphæa Tucumcarii* of the Pyramid Mount section. At Kent, in Texas, the superior part of the Tucumcari division exists with the *Gryphæa Tucumcarii;* and more of that division will be found in the region of the Pecos River valley and also on the upper Canadian River region. As I have already said, the strata of the Kiamishi Creek valley in the Choctaw Nation, containing the original *Gryphæa Pitcheri* of Morton, may belong to the lower part of the Tucumcari division.

C. We next come to a new great period, the Cretaceous age, beginning with the strata containing an immense number of *Gryphæa Rœmeri*. When I say that the Cretaceous begins with the apposition of the *G. Rœmeri*, it is only a means of calling the attention of the observer in the field, for there are some strata below the true zone of the *Gryphæa Rœmeri*. For instance at Comet Creek, Oklahoma, we have one bed containing *Caprotina Texana*, with a form of *Exogyra Texana;* while at Black Hills, Kansas, we have thirteen feet of shales, numbered 7, 8, 9 and 10 in the section of Mr. Hill, containing no fossils, which very likely are already Cretaceous strata. I recall that No. 6 of Mr. Hill's section belongs still to the Jura, and that denudation or erosion very likely exists between number 6 and number 7, the last number beginning there the truly Cretaceous series. The presence of "occasional minute pebbles" in No. 7, indicate the disturbance after the break occurred at the end of the Jura, and is an important witness of the denudation and erosion.

With the *Gryphæa Rœmeri* zone, we have the beginning of an important series of strata, containing a Lower Cretaceous fauna well characterized and which can be subdivided into four or five sub-fauna and easily followed and studied between Belvidere, Fort Washita, Fort Worth and Comanche Peak.

We have three *facies* of the Lower Cretaceous or Neocomian. One is called the Washita *facies*, composed mainly of limestone and constitutes the true Washita division, as I established it as long ago as 1853, when at Comet Creek, near Fort Washita. The second *facies* is called by Professor Cragin "Kiowa shales," being composed mainly of shales, instead of the great development of limestone of the Washita *facies*. The second *facies* has seemed until now to be confined to the strata of Kansas. Then we have a third *facies* called Fredericksburg division, at Comanche Peak, Walnut, Goodland and further south.

A great deal of confusion has been caused by a sort of duplication of one single series, the true Washita division, which has been called in eastern and southern Texas Fredericksburg division. A careful survey of all the strata composing the Lower Cretaceous between Fredericksburg, Austin, Comanche Peak, Fort Worth, Fort Washita and Comet Creek is much needed. For it is plain that the Comanche Peak and Kiamitia clay or Preston subdivisions are simply the base of the Washita division, which cannot be separated from the Fort Worth limestone.

Therefore, instead of the classification used by Mr. Hill under the single name of "Comanche series" or Lower Cretaceous, composed of these divisions, 1st the Trinity, 2d the Fredericksburg, and 3d the Washita, we have the Jurassic series composed of the Trinity and the Tucumcari division, and the Lower Cretaceous or Neocomian composed of the Washita division; dropping the Fredericksburg, which is only a *facies* of the Washita, and placing the Tucumcari division in its true stratigraphic and paleontologic position.

General tabular view for the whole country south of the Arkansas River:

Upper Cretaceous.

Break.

Neocomian or Lower Cretaceous.	C. Washita Division.	Comprising all the subdivisions, as well as the Kiowa and Fredericksburg *facies*.
		Zone of the *Gryphæa Kansana*.
		Zone of the *Gryphæa Rœmeri*.

Break.			
Jura.	B. Tucumcari Division.	Until now this upper part of the Jurassic formation has been best preserved in the Tucumcari region. A few remnants exist near Belvidere (Kansas), at Kent (Texas), and possibly in the Choctaw Nation. Zone of the *Gryphæa Tucumcarii* and *Ostrea Marshii.*
	A. Trinity Division.	Best developed in Pike County, Arkansas, and Bosque County, Texas. Represented in Kansas by the Cheyenne sandstone; at Pyramid Mount, New Mexico, by the yellow and white sandstone; and at Kent, Texas, by sandstone called Paluxy.

Resting on the New Red sandstone or the Palezoic series according to different parts of the country.

Cambridge, Massachusetts, May, 1897.

ART. XXIV.—*On Pithecanthropus erectus;* by Professor L. MANOUVRIER of the Paris School of Anthropology.*

Extracts selected from two articles: "The *Pithecanthropus erectus* and the Theory of Evolution" (Revue Scientifique, 4me sér., t. v), and "Response to the Objections against the Pithecanthropus" (Bull. de la Soc. d'Anthrop. de Paris, 4me sér., t. vii). Translated by George Grant MacCurdy, M.A.

THE Revue has already said a few words about an important scientific event which I now propose to discuss more fully.

It is a question of the discovery in Tertiary strata near Trinil, Java, of bones which seem to have belonged to a being intermediate between man and the anthropoids. This could be a precursor and perhaps an immediate ancestor of the human species, the link, heretofore lacking, of the chain which, according to the theory of evolution, ought to unite without interruption *Homo sapiens* with the rest of the animal kingdom. The author of this discovery is Mr. Eugene Dubois, physician in the Dutch army. The occasion was a vast geologic exploration made in Java, from 1890 to 1895, under the auspices of the government of Holland.

Such good fortune did not come to Mr. Dubois by hazard. He was attracted to the Indian archipelago in the hope of finding there, by means of important excavations about to be undertaken, the famous Missing Link theoretically foreseen, the existence of which should antedate Quaternary times. Certain hypotheses even considered the "Iles de la Sonde" as a possible cradle of the human race. Mr. Dubois, then, was guided by theoretic views; and if he has been fortunate in his research he has merited it by his competence as geologist and anatomist, also by the talent with which he has known how to turn his discovery to account.

It is all very well to find an inscription, it is another thing to decipher it. This latter task, as will be seen farther on, presented great difficulties which Mr. Dubois has overcome in a most creditable manner.

A very incomplete skull, two molar teeth picked up at a meter's distance from the skull, and a femur lying at some fifteen meters' distance, the whole enveloped in an earthy gang, very hard, and occurring in a bed which included other remains of a Pleistocene fauna to day for the most part extinct—such are the pieces of a more or less human appearance of which the specific determination was in question. It is obvious that

* Two illustrated papers on the *Pithecanthropus erectus* have already appeared in this Journal, both by Professor O. C. Marsh, who from the first regarded this new form as intermediate between man and the higher apes. See vol. xlix, p. 144, February, 1895; and vol. i, p. 475, June, 1896.—ED.

the inscription to be deciphered is far from being perfect; the letters remaining are few. But these are the initials, and in the connection in which they are found, chance has served science almost as well as a judicious choice among all the parts of the skeleton could have done.

The two molars represent, in reality, in addition to the maxillary bones and the face, the vegetative function; the femur represents the function of locomotion; what is left of the skull suffices to give important indications as to the cerebral and intellectual development.

Although these different pieces were found separated a certain distance one from the other, the conditions of the deposit and the circumstances of the excavations have convinced Mr. Dubois that they belonged to one and the same individual. He has made a thorough and very careful study of them,* of which the conclusion is that they attest the existence, in the Pleistocene epoch, of an anthropoid species of biped intermediate between the known anthropoids and the human species, precursor of the latter and probably descended from the genus Hylobates (Gibbon). In consequence, the new species received the name of *Pithecanthropus erectus*.

Strongly supported as they were, these conclusions were destined to move more or less, not only the specialists in zoology, anthropology, and paleontology, but also the entire thinking world. Appearing toward the end of the year 1894, Mr. Dubois' memoir was not slow in provoking criticisms and discussions, the history of which is not without some interest.

January 3, 1895, I communicated to the Paris Society of Anthropology† a detailed estimate, based upon the study of the drawings, photogravures and tables, contained in Mr. Dubois' paper, and which, in part favorable to the conclusions of the author, may be summed up as follows:

It is not certain that the specimens in question belonged to the same individual nor even to a single species, but it is possible, for there is no lack of anatomical correlation among the different pieces.

The femur, for the human species and according to my tables for the reconstitution of the stature‡ would correspond to a height of about $1{\cdot}657^{m}$. This femur, by its pilastric index§ or index of a transverse section at the middle point of the

* Pithecanthropus erectus, eine menschenæhnliche Uebergangsform aus Java (Batavia Landesdruckerei, 1894).

† Discussion du Pithec. erectus comme précurseur présumé de l'homme (Bull. Soc. d'Anthr., fasc. 1, 1895).

‡ Mém. sur la détermin. de la taille d'après les grands os des membres (Mém. Soc. d'Anthr. de Paris, § 2, t. iv, 1892).

§ Étude sur les variations morph. du corps du fémur dans l'espece hum. (Bull. Soc. d'Anthr., 1893.)

diaphysis, indicates certainly a biped attitude. But it does not present a single character permitting one to attribute it to any other species than the human. Yet that invalidates in no way the general conclusion of Mr. Dubois, because on the hypothesis, where an anthropoid race would have passed from the attitude of a climber to the attitude biped, the transformation of the femur ought to have preceded that of the skull.

The tooth (3d upper molar) is too large sized, its roots too divergent to admit of its being attributed to man. I have been able to find only one human tooth (in a New Caledonian skull) which presents at once so large a crown and of which the principal axis is at the same time directed from before backward, but this is a third lower molar and its roots are not spreading. On the other hand, the grinding surface of the fossil tooth from Java differs much from the known teeth of anthropoids. It should then be considered as having belonged either to an anthropoid race, or to a human, no longer living.

The skull, according to Mr. Dubois' calculations confirmed by my own, has a capacity of from 900 to 1000^{cc}. This capacity exceeds by about 400^{cc} the maximum found among the largest anthropoids. On the other hand, it is too small to be compatible with a normal human intelligence, save among individuals of very small stature having a cranial capacity relatively large with reference to their stature and with reference to the average of their race. But, even discarding the teeth and femur about which there is some doubt, the morphologic characters of the cranium from Java suffice to denote a cerebral volume relatively very weak. The skull then must have belonged either to a normal individual of a race intermediate between the grand anthropoids and man, or to an abnormal man, to an imbecile, microcephalous for his race. This last supposition has the disadvantage of admitting the extraordinary encounter of an anomaly; if such an encounter is, strictly speaking, possible, it is hardly probable. In short, at least, a skull morphologically intermediate is in question. It is not certain that this skull represents the normal state of a fossil human race equally intermediate, but it is still less certain that it is a question of a simple anomaly. Consequently, the hypothesis of Mr. Dubois is scientifically legitimate.

Such were my first conclusions in January, 1895. But very different conclusions were reached at about the same time in Germany and in England.

At the Berlin Society of Anthropology,* the question was examined by Kraüse, Waldeyer, Virchow, Luschan, and Nehr-

* Zeitschrift für Ethnologie, Heft i, 1895.

ing. There, the femur was declared human and the skull attributed more or less affirmatively to an anthropoid.

On the other hand, Cunningham at Dublin and Sir W. Turner at Edinburgh pronounced both skull and femur human; Rudolph Martin* of the Zürich Univerity was of the same mind.

Such a divergence of opinions expressed by these anatomists, all of them so competent, would almost suffice to demonstrate the really intermediate state of the skull from Java, for it is well known how great the difference is between a human skull and that of a monkey. To give occasion for opinions so opposed, it was necessary that the skull from Java should present important characters human and important characters simian.

That which explains also the divergence in question, is that the human skull drops now and then to a simian level among the microcephalous of all races, and to a level approaching the Pithecanthropus among certain inferior individuals, especially in the lowest savage races.

The skull from Java is no less remarkable in its general form than in its weak capacity. Its entire median curve is extremely elliptic; the forehead is extremely narrow and taper-

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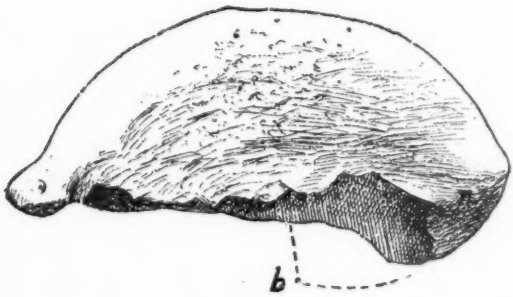

FIG. 1 (fig. 53).—Profile of the cranial cap of Trinil; *b*, Approximate position of the basion; *n*, Rudiment of the temporo-occipital crest.

ing. The lower portion of the frontal bone above the orbits forms a sort of visor of which the relative prominence surpasses all known proportions in the human species, not excepting, even, the famous Neanderthal skull. The lateral projection of this visor is not less extraordinary and denotes a great depth of the temporal fosses. The frontal region presents a

* Kritische Bedenken gegen den Pith. erect, Dubois (Globus, Band lxvii, No. 14.)

lateral flattening which gives to the ensemble of the cranium a pyriform aspect when seen from above. The posterior parietal region is flat from above downward to a degree no less remarkable. The occipital crest is very thick. The temporal crests do not come very near to the sagittal suture, but they are prolonged downward and backward in such a way as to form a parietal super-mastoid crest which goes almost to form a junction with the occipital crest. I at first pointed out with some reserve this simian character after a photogravure in Mr. Dubois' memoir; but I am now no longer in doubt as to its reality. Finally the foramen magnum and the auditory meatus, which are missing, appear to have been situated a little farther back than in the human species.

As has been said above, human crania very inferior for their race sometimes approach more or less in volume and form to anthropoid crania. Professor Turner* has also been able to show many exceptional human skulls that approach to a remarkable extent the skull from Java with reference to capacity, etc. But, if we suppose that collections of crania richer than those that we possess would permit us to find upon human crania all the characters of inferiority noticed on the Pithecanthropus and to a degree as pronounced, the skull from Java would present none the less this peculiarity: that it brings *together a group of characters all of them the limit for the human race.* It is the union of these characters that it behooves us to consider, all the more so that the coexistence of certain of these characters on the same skull is particularly interesting. Thus normal human crania can have an inferior capacity of 1000 cubic centimeters, but then these are pigmy crania, and they come up again with respect to the general form because they contain a brain relatively voluminous with reference to the stature; they have no right, so to speak, to that enormous frontal visor which is, among all races, the lot of individuals with powerful skeleton and brain *relatively* small, or of averred microcephalous individuals whose development of skeleton approaches the medium.

Besides, let us admit that a non-pathological human skull may be found in which are united all the "*caractères limites*" of the skull from Java; that would prove nothing against the hypothesis of Mr. Dubois, for such a skull would be always a very rare exception in any human race whatsoever, whereas, according to all probability, the one skull found in Java is not a rare exception in its race. And then this race is of the Pleistocene epoch, which of itself would give no ground for astonishment were its one known specimen morphologically

* Jour. of Anat. and Physiol., vol. xxiv, 424.

inferior to our present races. We will refer further on to this question.

The opinion expressed in Germany is explicable, in the first place, by the fact that they commenced by attributing the femur of Java to a man without further question. In the second place, they emphasized too much the simian characters of the cranium and teeth. They saw that, according to these characters, the race of Java could not be attributed to the human species, but forgot that, according to other characters, they had no right to attribute it to the race of monkeys. For no known anthropoid approaches the fossil race of Trinil either by its cranial capacity or by its occipital characters at an adult age.

Besides, a view of the remains themselves and a more thorough study of them have already resulted, so it seems, in a change of the opinions first expressed.

Be that as it may, Mr. Dubois can congratulate himself on seeing placed in relief, at Berlin, the reasons according to which his *Pithecanthropus* could not be a man and, in England, much better reasons according to which the same *Pithecanthropus* could not be a monkey.

The question rested there until the International Zoölogical Congress was held in Leyden, September, 1895. At this congress, where were found such eminent zoölogists and anatomists as Sir W. H. Flower of London, A. Milne Edwards, Perrier and Filhol of Paris, and others, Mr. Dubois showed the fossil pieces from Trinil, to which was added another tooth (2d molar) which he mistook at first, before having completely cleared it from its matrix, for a tooth of *Suidæ*. The view of the originals did not result in calling forth decided affirmations from the Congress. According to the information that I received from Mr. Dubois and from Professor Kollmann of Bâle, and according to a communication made to the Paris Academy of Sciences by Milne Edwards, the question was considered as demanding further research. Professor Virchow, without committing himself, emphasized certain pithecoid characters of the skull and femur, notably the resemblance of the femur to that of the gibbon; he showed especially that, according to researches made in the collections of Pathological Anatomy of Berlin, the voluminous osseous vegetation presented by the femur of Trinil in the posterior sub-trochanterian region might be due to an abscess from congestion of the thigh, probably following a caries vertebral.

Mr. Dubois having satisfied himself, at Leyden, that the direct view of the fossil remains from Java contributed much to corroborate his demonstrations, kindly took those remains first to Brussels, then to Paris, then to Dublin, to Edinburgh,

London, Berlin and Jena. He will set forth, without doubt, in the near future the happy effects of that scientific tour upon anatomists when he describes the fossil fauna (Upper Pliocene) contemporaneous with the *Pithecanthropus*. The opinion adopted seems to be to-day very generally analogous to the one set forth at the beginning of this article.

It is the aspect of the specimens from Trinil and their complete

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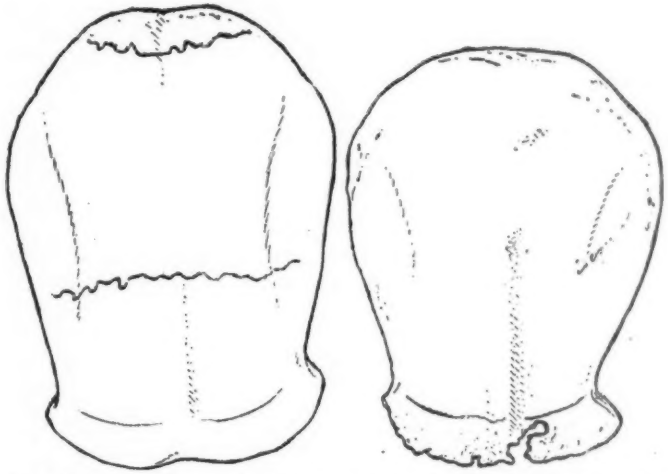

FIG. 2 (fig. 54.)—*Norma verticalis* of the skull of Trinil compared with that of the Neanderthal skull.

fossilization, which surpasses by far that of all human remains, even the most ancient known until then, that tend more powerfully than all demonstration to place them as contemporaneous and as coming from one and the same individual, especially since there exists among them no want of anatomic correlation. The degree of fossilization is such that the femur attains the weight of 1 kilogram, whereas prehistoric femurs of the same size do not exceed 350 grams. All that, joined to the conditions of the deposit, adds value to the divers anatomic facts and deductions given above, and constitutes a mass of arguments before which it becomes difficult not to surrender. Without doubt, these diverse fossil pieces, which all present characters intermediate between the human and the simian morphology, these diverse portions of the skeleton which are all explained the one by the other, do not come from two or three different species which would have met in some way by

special appointment within a space of a few meters to leave there, one its skull, another two teeth, and a third its femur, the whole without want of correlation.

As regards the femur, I have studied particularly the character upon which Mr. Dubois insists, viz: the almost cylindrical form of this bone in the poplitic region, 4 centimeters above the upper margin of the condyles. At this level, the transverse diameter is ordinarily much greater than the antero-posterior diameter. On the femur of Trinil these two diameters are almost equal. At the same time, if we measure, beginning at a point anterior *m*, two antero-posterior diameters, the one ending at the median point *p*, the other at the point *n* situated on the external branch of the bifurcation of the linea aspera, we find $mn < mp$. I have been able to find this double character on only six human femurs out of more than a thousand belonging to races very diverse. Again it is less accentuated than on the femur from Java, so that this femur presents also in this respect a new "*caractère limite*" for the human species. One human femur alone presented this character to the same degree as the femur of Trinil; it is a Parisian femur of the Middle Ages, and this bone is pathologic; it presents grave coxalgic lesions and divers characters attesting a consecutive functional impotence. A detailed interpretation of the character in question is found in the *Bulletin de la Société d'Anthropologie* (t. vi, 4me sér.). It can be summed up as follows:

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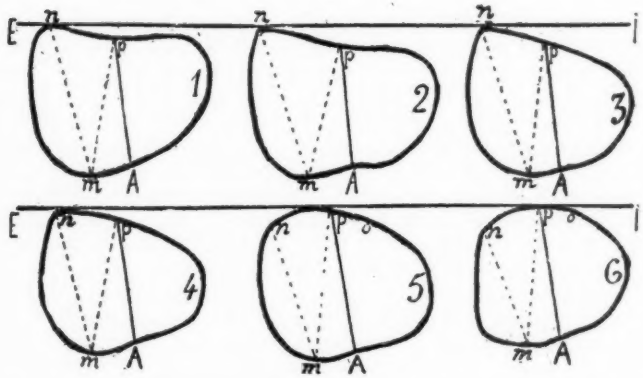

FIG. 3 (fig. 55)—Transverse section of the femur 4 centimeters from the upper margin of the condyles.—Scheme representing the passage from the common type 1 to the form of the femur of Trinil 6.—EI, Transverse axis.—Ap, Antero-posterior axis.

This character can be produced sporadically in any race whatsoever; it does not seem to possess any ethnic value in the human species, it seems to be connected most often with a certain muscular weakness and can be the result of a lesion affecting the upper part of the bone. As the femur of Trinil presents exactly such a lesion, resulting itself from a malady capable of entailing during a period of years a relative impotence of the lower members, it is quite possible and, I believe, even probable that if we should find a second femur from the same race, it would be very different from the one we possess.

This has none the less a very great importance, because it attests peremptorily the "marche bipède" which the cranial characters had been powerless to demonstrate in a sufficient manner, and the rather large size of the subject. It is sufficient for us to know that the femur of Trinil is not that of a monkey but that of an animal maintaining the upright position, an idea which is not in the least disturbed by pathologic considerations. If the femur in question had been completely sound, its form would have approached even more the ordinary human form. Such as it is, it does not recall, in my opinion, the femoral form of the gibbon any more than the Quaternary femur of Spy, described by Mr. Fraipont, recalls the femoral form of the gorilla, provided one does not take into consideration the characters connected with an upright position. In other words, the femur of Spy, although human, would not be less pithecoid than that of Java.

Mr. Hepburn* of Edinburgh does not regard the characters of the femur of Trinil as sufficiently pronounced to form a genus distinct from the genus *Homo*. These characters are human and not simian. Upon this point, we are in accord. He adds that if the femur comes from a human being, and if the teeth and skull belong to the same, then the conclusion relative to the femur should apply also to the skull and the teeth.

On this last point, the justice of the conclusion depends on the signification attached to the term *human being*. If the femur of Trinil, considered separately, proves that its possessor was not a monkey, it certainly does not prove that its possessor ought to be classed according to the totality of its conformation in the human species, or genus, so far as known. I have already insisted at length upon this fact, that the femur can be morphologically very human in a being low enough with respect to cranial development to merit only conventionally the name of man. It is necessary, then, to take into account its skull and its teeth, as well as its femur, in an estimation of

* Journal of Anatomy and Physiology, vol. xxxi.

the fossil individual from Trinil. According to the femur, it would be a man with a perfect title to the name; according to the skull and the teeth, it is a creature low enough, in relation to the lowest human races, to be considered as passing beyond the lower limit for the human species or genus, so far as known, in the measure that its inferiority represents the inferiority of its race. It is this last point that rests in the condition of a hypothesis, but of a hypothesis which has for itself the greatest possibility. This hypothesis admitted, we are obliged to agree, viewed from the point of view of the theory of evolution, that the individual of Trinil, incontestably hominian, presents an ensemble of anatomic conditions responding marvelously to that which the theory of evolution could look for in an ancestral race.

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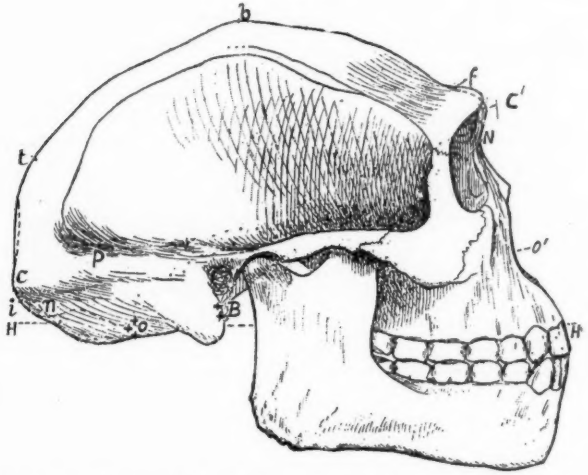

FIG. 4 (fig. 56).—Attempt at the reconstruction of the skull of *Pithecanthropus*.—B, Basion. The points marked about the letter B indicate the limit of possible errors.—C, Occipital crest.—*pt*, Inferior parietal crest almost joining the occipital crest.—*i*, Inion.—HH′, Horizontal plane of Broca (alveolo-condylian).—BA, Basio-auricular line.—BO, Plane of the foramen magnum.

As regards the skull, I have been able, by virtue of the cast kindly given to the Anthropological Laboratory by Mr. Dubois, to attempt the graphic reconstruction. I made the attempt simply to satisfy myself of the aspect resulting from diverse craniologic proportions, but I believe I have obtained a drawing conforming approximately enough to the reality to be of interest to anatomists.

The maximum error possible for the separate points of the skull has been limited by correlations so diverse and

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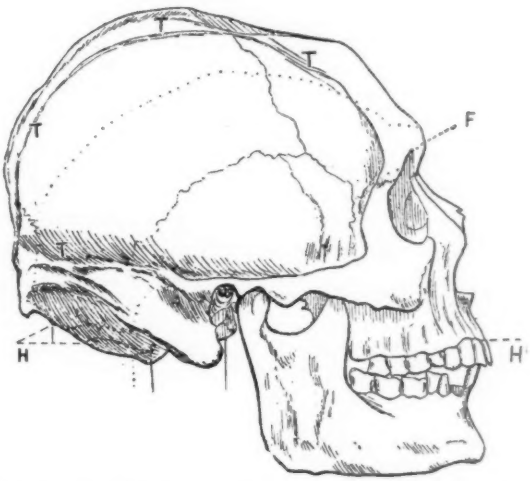

FIG. 5 (fig. 57)—Skull from Turkestan of bestial aspect in which is inscribed, by means of a dotted line, the cranial cap of Trinil.—IF, Inio-orbital line common to the two skulls.—T, Curved temporal line of the skull of Turkestan. This skull is extremely remarkable for the extent of the surface of insertion of the temporal muscle. It presents the crest *pt* of figure 4 (56). The sincipital region is mutilated by a sabre stroke.

rigorous that the errors which could have been committed cannot modify them, notably the general form of the skull,

6.

FIG. 6 (fig. 58)—Skull of a young chimpanzee.

properly speaking, and its orientation. The figure thus obtained seems to me to make it evident that, it is impossible,

with the cranial cap of Trinil, to construct a skull having an appearance either completely human or completely simian. The occipital characters which I attributed to it differ radically from those of adult anthropoids; in vain did I orient it in

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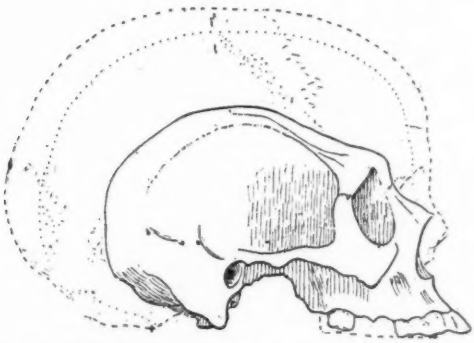


FIG. 7 (fig. 59)—Skull of *Margaretha* Moehler, microcephalous adult of Carl Vogt. The dotted lines represent two well developed feminine crania from Paris; one large, the other small. The auditory meatus is the point of superposition.

superposing it upon a human cranium; it had not, for that, an appearance suitably human, and we attempt in vain to pivot it about its bi-auricular axis in order to give it an air more human or more simian: we are struck by divers incompatibilities. The truth, which, I think, will appear clearly to all craniologists, is that the skull from Trinil represents the morphologic stage of the young anthropoid, a stage during which these animals approach man in important cranial characters much more nearly than at the adult age.

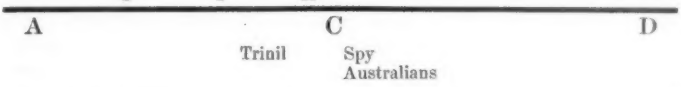
The adult *Pithecanthropus* possessed these characters of the young anthropoid; such is the result of our attempt at reconstitution, result independent, I repeat, of incurring chances of error, independent also of any preconceived idea, for I have striven only to place each separate point of the cranium and each line conformably to anatomic correlations without preoccupying myself as to the final result. It has been admitted that the two molars, the femur, and the skull belong to the same individual, but this hypothesis has not exercised the least influence upon the drawing of the cranial region, properly so called. The technical details and justifications are to be found in the *Bulletin de la Société d'Anthropologie* above mentioned. I present here only a few drawings, which may be compared advantageously with the preceding.

The fact that the skull from Java bears such a strong mor-

phological resemblance to that of a young anthropoid is of a nature to explain the divergence of opinions, some of which ascribe it to a monkey, others, to the human species. But as it is a question of an adult, this fact is pronouncedly in favor of ascribing the skull to the human species with the reserve that it occupies a rank morphologically intermediate between anthropoids and the lowest human races. However, an anthropoid maintaining the upright position and possessing such a cranium is nothing less than a low order of human, for it has lost the essential traits which differentiate man from anthropoids. It is understood in this sense that the opinions of Turner and Cunningham do not differ from mine.

The important thing is the establishing of the fact that the craniologic inferiority of fossil human races, according to the specimens we know, increases with their antiquity. The discovery of Mr. Dubois contributes to establish this fact.

Let us represent by a line AD the entire family of Hominidæ, which, for the theory of evolution, includes, in addition to the genus Homo in its known state CD, an unknown fossil portion CA, connecting the known portion with an anthropoid ancestor whatsoever A. When we say that the individual from Trinil belongs to the human species, that signi-



fies that it can enter into the portion CD within the limit L, which, for the anti-evolutionists, the human species must not overlap.

When it is said, on the contrary, that the race of Trinil is inferior to all known human races including the portion C, it is considered thereby even as one of those intermediate races TT' which, according to the theory of evolution, ought to have formed the unknown portion of the line AD.—Whether or not we place this race under the genus *Homo* (which is of little moment for the evolutionist), we consider it as one of the intermediate fossils theoretically foreseen. To contradict this opinion and to attach the man of Trinil to the race of Spy is to admit that it is a question always of the portion CD representing without theory the species or genus Homo. Such was the former opinion of Turner and of Cunningham; opinion which has been perhaps modified since the direct examination of the specimens under discussion.

According to the contrary opinion, the *Pithecanthropus* represents one of those fossil human races that the theory fore-

saw, for it is morphologically intermediate, by its skull, between the lowest human races and the anthropoid type. A partisan of the theory of evolution has no repugnance to considering that race as human and to saying "the man of Trinil," since, according to the theory, the chain AD is necessarily uninterrupted. Whatever be the names that we shall judge proper to give to the divers links of this chain, it will be a question always of man more or less inferior as far as the point where, the type of "bipède marcheur" disappearing, we shall emerge from the definite family of *Hominidæ* to enter into another branch of the genealogic tree of Man.

If it is preferred to settle the question by saying that the skull of Trinil simply puts back the limit L beyond its present position, just as the skulls of Spy have extended this limit as regards the races of Europe: from what I have just said I should not see the slightest objection to that, since it seems to me this limit L is destined to be put back by successive degrees as far as to the level A.

Theoretically it is highly probable that an anthropomorphous species, evolving toward the human type, ought to have realized at first in the adult state the characters of superiority that it possessed transitorily in the young state before that evolution. The disappearance of these infantile characters of superiority results, as I have shown in a former memoir,* from the precocious arrest of development in the cerebral mantel, when the central and inferior encephalic region as well as the basilar region of the skull continue to grow, keeping pace with the general development of the body. The *Pithecanthropus* would represent then that inferior phase of human evolution in which the intellectual and cerebral improvement would have been just enough so that the development of the upper portion of the skull would not be left in arrears any more than it is in the young anthropoid compared to the basilar development correlative to the corporeal growth in general. Among the lowest existing human races, this stage of evolution is largely exceeded for the normal individual. The difference is yet greater for the average among European races.

At any rate, the quality of precursor attributed by Mr. Dubois to his *Pithecanthropus* reposes upon an ensemble of facts of consequence enough to merit the most serious attention. In addition, behind this hypothesis there arises another to the view of the evolutionist. It is quite natural to propose the question whether the precursor were not something more, that is to say an immediate ancestor of man or of a part of the human species.

* Sur les modif. du profil encéphalique et endocr. dans le passage à l'âge adulte, etc. (Bull. Soc. d'Anthr. de Bordeaux, T. 1, 1884).

The hypothesis of a simple precursor can be accepted without repugnance independent of the doctrine of evolution. It simply places an intermediate species between anthropoid and man and confirms once more the adage: *Natura non facit saltus.* It reduces itself to a simple verification. In favor of this hypothesis there will be, on the one hand, all the arguments produced to demonstrate that it is a question of the anthropoid species, but veritably simian, until then unknown; and on the other hand, all the arguments produced to demonstrate that it is a question of the human species.

The hypothesis of a veritable ancestor will profit by all these arguments, for all will tend to establish the existence of an uninterrupted chain. In insisting upon the simian characters we strengthen, voluntarily or not, the affiliation of the *Pithecanthropus* with monkeys; in insisting upon the human characters, we render more probable the affiliation of the intermediate species with the human.

The scientific event due to the laborious researches of Mr. Eugene Dubois is of a nature to give joy to all friends of science, but it seems to be more particularly agreeable to evolutionists, that is to say, to those who desire and pretend to explain why *natura non facit saltus.* For these last, the question whether the *Pithecanthropus* ought to be classed with the genus *Homo sapiens* depends upon the value attached to the qualifying word *sapiens*, the value of which is already very relative. As to the question of species it is, for the evolutionist, like the preceding, a simple question of degree of morphologic differentiation.

It is none the less interesting to search for the particular simian genus to which would fall the honor of becoming founder of the human branch, in other words the known anthropoid genus to which is allied the intermediate *Pithecanthropus.*

Mr. Dubois has thought of the genus *Hylobates* (Gibbon) and the general opinion at present seems to accord with this view. All the appearances are in its favor, because of the relatively grand analogies which exist between the conformation of the gibbon and that of man.*

The almost vertical attitude of the Gibbon corresponds to the very marked anatomic particularities which would render easy the human transformation. The conditions of this transformation, that is to say of the passage from the state of climber to that of "marcheur bipède," ought to have been very imperious, for it is difficult to believe that, without that, a race of climbers took spontaneously the initiative in renouncing a

* Paul Broca: L'ordre des Primates (Bull. de la Soc. d'Anthr., T. iv, p. 228, 1869).

mode of locomotion in harmony with an adaptation instinctively and organically fixed.

One hypothesis among others would be the destruction, more or less complete, of the forests on an island inhabited by anthropoids capable of taking, when necessary, the biped attitude. The ancient volcanoes of Java might have accomplished this destruction and have rendered necessary the adaptation to the upright position under pain of extinction of the race.

It would be impossible to explain easily the disappearance of an anthropomorphous species as much superior to all others as was that of the individual from Trinil; for it was strongly built with a cerebrum superior to all known species of the order of Primates. It possessed, then, excellent chances of survival in the struggle for life. But, on the hypothesis here considered, the species *Pithecanthropus erectus* would not have disappeared. Having become a human race, it could not remain at the same time a race anthropoid. If the *Pithecanthropus* was only a simple precursor, it was superior enough to the other animals to survive unless the human species, springing up all of a sudden, "from the clay of the earth," did not hasten to annihilate this dangerous competitor. But if the *Pithecanthropus* was an ancestor, its species lives yet in its human descendants.

The difference between the *Pithecanthropus* and existing man is so small that there is no call to search for an intermediate link. The link is sufficiently represented by the lowest of our savage races; for example, the isolated human skulls, Australian and others, that have already been shown to be very little different in many respects from that of Trinil.

Supposing that among several species of gibbon, Gx, Gy, Gz, this latter species evolved toward the human type and became finally, in assuming the upright position, the *Pithecanthropus erectus*=H^1, then that it, by virtue of the multiple consequences of the upright position, became progressively H^2, a stage corresponding to the lowest existing races, we obtain in simplifying:

Gibbon x
Gibbon y
Gibbon z—H^0—(P.E.=H^1)—H^2.

There ought to be then in the existing fauna a hiatus formed by the transformation of the gibbon z into H^0, then of H^0 into H^1, so that, in this existing fauna, the species nearest to H^2 ought to be a species very inferior, issue of gibbon x or y. The gap here ought to be all the greater in that it is not only a question of a transformation such as that of one quadruped into another, conserving the generic characters of its ancestor;

but of a transformation of the attitude even, that is to say of morphologic conditions entailing a radical change of type and, indirectly, of physio-psychologic modifications very profound.

The existence of a hiatus between two related living species cannot then serve as argument against the theory of evolution. This hiatus, as we have just seen, may be, on the contrary, a direct result of the transformation of one species into another.

Although the transformation here supposed has been very profound, enough so to give birth to a pretended new *kingdom*, "human kingdom," that transformation could have been produced, according to the above hypothesis, without compelling Nature to make, in any sense, a leap. It may be possible, from a point of view purely zoötaxic, to establish a veritable *saltus*, but I have just shown that this saltus could have been the gradual consequence of a simple modification of habits of locomotion in a race of monkeys already capable of assuming the upright position. The motive for this change could have arisen abruptly, but there has been no anatomic leap from Gibbon *z* to existing man. That which can have been produced abruptly is the exterior condition from which would have resulted, for an anthropoid race of climbers, the necessity of adopting habitually a mode of locomotion which it was already capable of utilizing occasionally. The only thing abrupt, from a biologic point of view, would have been a simple increase in the frequency of the utilization of a functional aptitude already existing. Multiple and considerable anatomic modification may have been entailed by this change of the habitual attitude, but they ought to have been produced by insensible degrees and are all the less astonishing in that the anthropoids already approach much nearer to man than to monkeys proper in their general conformation (Huxley, Broca).

If there is a gap between the existing human species and the precursor, the fossil remains of the intermediate races ought none the less to exist. There ought to be the remains of H°, of gibbon *z* and of *Prothylobates*. Will these last perhaps reveal a species remarkable in stature and in a relatively superior aptitude for the upright position? That is not necessary theoretically: the diverse species of the genus Hylobates have a conformation which enables them to assume the upright position with ease; the form may have undergone considerable variations after the transformation of the attitude.

Finally, it is probable that the species gibbon *z* approached man in certain respects more than do known species of the genus Hylobates.

However, if we admit that the pieces found at Trinil really represent the remains of a *Pithecanthropus*, and if it is admitted

that this was an ancestor of man, it is necessary to find now an ancestor to this *Pithecanthropus*, and it seems requisite that this ancestor be not inferior to existing anthropoids. It must have been capable of adopting, in case of need, the upright position, and been led, by its conformation, to take that attitude rather than the quadruped attitude. Such would be certainly the case with all known anthropoids, all of which are veritable biped *climbers*.

Let us recall here the existence in the Miocene epoch of several anthropoid species such as the *Dryopithecus*, the *Pliopithecus*, and the *Anthropopithecus sivalensis*. As Mr. Dubois has remarked, his species does not lack for ancestry.

The transformation of the habitual mode of locomotion may have been very rapid, but the consecutive, morphologic transformations must have demanded much time and cannot have been fixed hereditarily until after a certain number of generations—hundreds perhaps, and perhaps many less, for selection under the conditions indicated above may have been very active; the two sexes must have contributed actively to the progression, and the young must have imitated their parents with an ever-increasing facility. As regards the *direct* morphologic consequences of the change of attitude, we may suppose they were produced with great rapidity, if we are to judge from the multiple skeletal variations caused in man under the influence of the minimum of functional variations compared with those with which we have to do here.

As regards cerebral increase, it proceeds with such slowness that we can scarcely affirm the fact has been established at all for our European races since prehistoric times. But the cranial capacity of the *Pithecanthropus* surpassed by about 300 grams that of the largest gorillas. It surpassed by at least as much that of its ancestor gibbon *z*, if this latter was of the same stature as the *Pithecanthropus*. There is here an enormous difference, greater than that between the average for our lowest and the average for our highest existing human races. It is not, however, embarrassing for the hypothesis under discussion.

We must consider, in fact, that the human species has never realized, since the beginning of its existence, a progress comparable to that represented by the passage from the state of climber to the state of "marcheur bipède." This passage represents a veritable liberation of the superior members, the hands, previously employed as organs of locomotion the same as the feet. It is by the mode of locomotion of the climber that the hand became, little by little, apt for the function of prehension, then for the function of manipulation, and it is by virtue of the complete emancipation here supposed of the

superior member with reference to locomotion that the functions of prehension and of manipulation of the hand have been able to acquire adaptations the most varied. The perfecting of the tactile sense must have been an immediate result of this emancipation. This result must have involved the acquisition of a multitude of new notions suggesting new movements, new actions. From that, the multiplication of the movements of the fingers and of their combinations, the increase in manual skill and all the psychologic consequences, reacting the one upon the other, which must have been produced necessarily, by increase, in variety and complexity, of newly acquired motive and sensorial representations. On this subject, I could not do better than to refer the reader to the beautiful pages devoted by Herbert Spencer to the parallelism of the sensorial and motor improvement in the animal series together with the intellectual improvement.*

It is impossible to say, even approximately, to what augmentation of cerebral weight the transformation in question may correspond, but there are grounds for believing that this augmentation must have been considerable, all the more so since the intellectual growth in question must have influenced simultaneously the sensorial and motive manifestations, and the order of sensations the psychologic importance of which is extreme, and the order of movements (the movements of the fingers) very numerous and which we know to be of great help in the function of expression. This function is perhaps the most important to be considered here, because its progress reacts in a capital manner upon intellectual and social development. It may have been noticed, among divers savage peoples, how much the language by gesture makes up for the imperfections of the spoken language; it is then allowable to suppose that the movements of the hands and of the fingers figured largely among the primitive means of expression of Pliocene man.

I do not believe it is possible to cite any ulterior cause of psychologic progress and of increase in brain weight comparable to the emancipation of the superior members with which we have just been occupied. The perfecting of articulate language must have been consequently the principal factor supervening in the psychologic and cerebral progress, to which would be due the superiority of the lowest existing races over the *Pithecanthropus*.

The quantitative cerebral progression has been accompanied by an improvement in the general form of the brain. This improvement is already perceptible in the *Pithecanthropus* according to the general form of the skull; it seems, however,

* H. Spencer, Principles of Psychology, vol. i.

to have been about parallel to the quantitative progress from the anthropoid precursor to civilized man. But it is not possible to introduce here this very complex question with the necessary developments.

It would not be absurd to try upon the gibbon an experiment conformable to our hypotheses. Without going so far as to wish to reproduce the formation of a new *Pithecanthropus*, we might attempt to picture what would happen to the attitude in placing the gibbon under conditions favorable to the transformation of its habits of locomotion.

As an intermediate form between man and monkeys, it is difficult to image anything more satisfactory than the skull of Trinil. If this skull, as is probable, is not exceptional for its race, we can count upon finding other specimens approaching still more nearly, either to man or to the monkey. But what the race of Trinil has not yet furnished, have not the lowest human races furnished in abundance? Do there not exist human crania, inferior compared with the average of their race, which show to us all the transitions theoretically desirable between man and the *Pithecanthropus?* All the inferior human crania which it would be possible to show as approaching the form of Trinil by certain characters would make up very well for the absence of the better specimens of the race *Pithecanthropus.* But it will be difficult to find, among normal human skulls, specimens as pithecoid as that of Trinil. We see frequently in a race such and such individual characters recalling an ancestral type, for it is easier to descend than to ascend in matters of evolution; but the pathologic arrests of development supervening during the embryonic stage only are capable of giving rise to a whole ensemble of characters recalling a remote phase of phylogenic evolution. Microcephalous idiots only, even among the lowest human races, present such an ensemble of characters which come to realize a morphologic type inferior to that of *Pithecanthropus* itself.

The distance existing between the *Pithecanthropus* and normal man must be considered as a necessary result from the point of view of evolution. It is the superior portion of the intermediate race which can have survived and formed an inferior human race. This latter must then present characters superior to the average of its ancestors, even independently of the progress that this human race can have realized since the Pliocene epoch. The existence of human crania presenting at once the ensemble of the cranial characters of *Pithecanthropus* has not yet been demonstrated, unless we take into account the microcephaly more or less accentuated, that is to say, a veritable anomaly by arrest of development. But we cannot represent a race by an abnormal skull, and it will be noted in the present instance that the resemblance existing

between human skulls more or less affected by microcephaly and the skull of Trinil would not contradict the hypothesis according to which this last would represent an ancestral race. This resemblance, on the contrary, would be perfectly conformable to the theory of evolution, and it exists. Without going beyond civilized races, we know that complete microcephaly carries man back to a level with the monkeys. It is then solely the poverty of our collections which has kept us from finding, among the lowest human races, skulls as pithecoid as that of Trinil. The skulls presented by Sir W. Turner, in his interesting memoir on the subject, approach it only partially. It is the same with the Sambaqui skull which Professor A. Nehring of Berlin has just confronted with that of Trinil.*

Crania approaching more nearly to that from Trinil under the double aspect of form and of capacity will certainly be found, but they will be crania very inferior to the average of their race; they will be the microcephalous, the abnormals.

Nothing would better serve to show that the species of *Pithecanthropus* and the human species penetrate into each other and are mutually bound together. The bond would be still more complete if we should find some day, by virtue of inverse operation, a whole fossil series of the race *Pithecanthropus erectus*, of which the superior extremity would accord morphologically with the average of our lowest races.

To invalidate the legitimate and probable hypothesis of Mr. Dubois, it would be necessary to show that the skull of Trinil is a simple monstrosity without ethnologic signification. This chance would be mathematically possible, since the race of Trinil must have had, as others, its microcephalous individuals; and it is for that reason that the opinion opposed to that of Mr. Dubois can pride itself, until there has been further investigation, in one possibility as against thousands of contrary possibilities. The improbability of a case of submicrocephaly coincident with a stature at least medium seems to me still greater since I have seen the two molars of Trinil, for teeth too large and too long for a normally developed savage would attest, in case of human microcephaly, one more singularity; a microcephaly which would have exaggerated, not only the volume of the teeth with reference to the skull, but also the absolute volume of the teeth beyond the ethnic maximum.

The hypothesis of a case of microcephaly being cast aside, two others remain.

1st. During the Pliocene epoch, there lived in Java a human race intermediate between the lowest of known races and anthropoids.

* Ein Pithecanthropus-ähnlicher Menschenschädel, etc. (Naturwissenschaftliche Wochenschrift, 17, Nov. 1895.)

2d. During the Pliocene epoch, there lived in Java an anthropoid race possessing the "marche bipède," and intermediate, in cerebral development, between the highest forms of known monkeys and the human species.

We may fuse these two hypotheses into one, from the point of view of the theory of evolution, that is to say, we may consider with great probability the race in question not only as a race precursor for the human species, but also as a race ancestral, as the commencement of humanity.

That there is in all this much hypothesis, I do not deny. But the attributing of the pieces from Trinil to two or three unknown species closely resembling man, or to a single abnormal specimen of the human species, that is, also, merely hypothesis.

Then, since we are obliged to have recourse, in any case, to a hypothesis, we have to ask which one is the most suitable, not only to explain the facts directly on trial, but also to clear up this question henceforth thrust imperiously before us for examination, namely, what can have been the human species during the Pliocene and how can it have originated? In the presence of the discovery of Mr. Dubois, it is advisable to examine the question in its entirety.

The question does not admit of a mathematical demonstration, but there can be a degree of probability great enough to carry with it conviction. To admit as true, until there is proof to the contrary, a hypothesis which answers to a great number of facts without being contradicted by any, is to act in accordance with the scientific spirit. It has often been said that science does not consist in a heap, but in a chain, of facts. To discover this chain, hypothesis plays a necessary role. Certain zoologists suppose that the human species has had no ancestors. If this hypothesis, of which the probability is not of the first order, seems to them to be scientific and fruitful, the opposite hypothesis can boast of titles to belief at least equal, in our opinion. And if the human species did not appear by spontaneous generation,—if, on the other hand, the cranial characters of Quaternary man found in Europe represented a phase of evolution very little removed from the existing phase, there is cause for believing that there would be found in Pliocene deposits a race morphologically inferior to that of Neanderthal and of Spy. But this is precisely what has happened. The anthropomorphous human race, if you choose to call it so, found by Mr. Dubois, presents characters such that it may have resulted directly and progressively from the transformation of a race of anthropoid climbers. Under these conditions, if the doubt on the subject of the simian origin of Man is only proportionate to the reasons of a scientific order capable of giving rise to it, it seems to me it must be a very slender doubt.

Art. XXV.—*The Titration of Sodium Thiosulphate with Iodic Acid;* by Claude F. Walker.

[Contributions from the Kent Chemical Laboratory of Yale University—LXV.]

This investigation was undertaken to determine the nature and limitations of the reaction between iodic acid and thiosulphuric acid, and to show the expediency of employing iodic acid in standard solution for the direct titration of sodium thiosulphate. Riegler* states that iodic acid is readily obtained in the pure state, that it may be accurately weighed out, and that a solution of it may be exactly made up to a desired strength and kept for a long time unaltered. He further states that when a solution of sodium thiosulphate is titrated with iodic acid the reaction takes place according to the equation,

$$6Na_2S_2O_3 + 6HIO_3 = 3Na_2S_4O_6 + 5NaIO_3 + NaI + 3H_2O,$$

under which circumstances no free iodine will be evolved until all the sodium thiosulphate has been oxidized to tetrathionate; the first drop of iodic acid in excess, however, will react with the sodium iodide that has been formed, and separate iodine, as shown by the equation,

$$5NaI + 6HIO_3 = 5NaIO_3 + 3H_2O + 3I_2,$$

thus furnishing an accurate means for determining the end point.

A careful repetition of the work of Riegler has shown that his conclusions are in a large measure erroneous. Thus, it has been found that the ordinary "chemically pure" iodic acid, purchased from reliable manufacturers, is likely to contain more than the theoretical amount of iodine, due probably to the presence of the anhydride, although iodic acid can be safely employed for standardizing when it is made in the laboratory by dissolving the purified anhydride, crystallizing out the acid, and drying over sulphuric acid. Such a carefully prepared product, if used immediately, will be found to contain the theoretical amount of iodine. Riegler's proposed method of titration depends on two different reactions, and to insure the accuracy of the process these must be definite, complete and non-reversible under the conditions of analysis. Thus one molecule out of every six of iodic acid should be reduced by six molecules of thiosulphate, with the formation of a neutral mixture of iodide and iodate, free from other oxidizing or reducing substances. Under these circumstances it might be expected that iodine will be liberated by the first trace of iodic acid in excess. It has been found by investiga-

* Riegler, Zeit. für Analyt. Chem., xxxv, 308.

tion, however, that although the main reaction between iodic acid and sodium thiosulphate may result in the formation of sodium tetrathionate in the proportions given, there is nevertheless striking evidence of some other obscure action of the thiosulphate, which influences the reduction of the iodic acid in such a way as to make it impossible to calculate the analyses according to Riegler's reaction. Moreover, a peculiar "after-coloration" which invariably follows the first formation of the starch blue during the titration of one solution against the other, seems to point to the possibility that the reaction between the iodide and iodic acid is dependent, under these circumstances, on conditions of time and mass for its completeness. It is not impossible that some third compound of iodine, unstable in its nature, may be formed as an intermediate product and thus delay the liberation of iodine. In consideration of the results that have been obtained it appears that Riegler's proposed process for standardizing sodium thiosulphate, as well as his related method for the analysis of iodides,* must remain impracticable unless they can be modified so as to do away with a number of sources of error.

The analyses of solutions of iodic acid, during the entire course of the work, was invariably performed by adding to the portion of the solution to be analyzed an excess of potassium iodide, acidifying with 5^{cm^3} of dilute (1 : 3) sulphuric acid, and recovering the liberated iodine by directly titrating the acid solution with sodium thiosulphate, or by neutralizing with potassium bicarbonate in excess, and directly titrating the alkaline solution with arsenious acid. In the latter case the neutralization was performed in a trapped Drexel washing bottle such as has been described in connection with the analysis of iodides.† In either case one-sixth of the iodine recovered was calculated to iodic acid, according to the terms of the equation,

$$5HI + HIO_3 = 3I_2 + 3H_2O.$$

It follows from these proportions that to bring the analyses within the range of the decinormal solutions ordinarily employed, the iodic acid taken for analysis must be restricted to comparatively small amounts. In the present work it was found convenient to analyze the iodic acid in quantities not much exceeding one-tenth of a gram, in which case the variation in the results in the same series is found to be almost inappreciable. In both variations of the process one or two blank analyses were invariably made, by performing the operation as detailed, except that no iodic acid was employed, and the correction of one drop of iodine thereby shown to be necessary to

* Reigler, Zeitschr für Analyt. Chem., xxxv, 305.
† Gooch and Walker, this Journal, iii, 293.

bring out the starch blue was uniformly applied in the analytical work.

To determine whether or not the purity of the ordinary iodic acid is sufficient to admit of its direct application in standard solutions, a series of experiments was made. Two different samples of "chemically pure" iodic acid were used. The first was in coarse granular crystals, and the second was in the form of fine powder. Quantities of both of these were dried in a dessicator over sulphuric acid to constant weight. Neither sample lost weight appreciably when left for a considerable time on the scale pan. A third sample of iodic acid was prepared by dissolving a quantity of the purest obtainable iodic anhydride in water, and evaporating at ordinary temperature. The resulting crystalline mass was dried over sulphuric acid in a dessicator for one week, until it ceased to lose weight, when it was presumed to consist of the pure normal acid. Two presumably decinormal solutions of each of the first two samples, and one such solution of the third sample of iodic acid were made by weighing out 17·585 grms. and dissolving in exactly one liter of water at 15° C. Convenient portions of each of these solutions were analyzed in the manner described, with results shown in the following table, averaged from many determinations.

TABLE I.

Analyses of Approximately $\frac{N}{10}$ *Iodic Acid.*

Solution analyzed.	Sample used.	HIO_3 taken. grm.	HIO_3 found. grm.	Error. grm.
I	A	0·1055	0·1066	0·0011 +
II	A	0·1055	0·1062	0·0007 +
III	B	0·1055	0·1065	0·0010 +
IV	B	0·1055	0·1073	0·0018 +
V	C	0·1055	0·1053	0·0002 −

These results show that while the deviation from the theoretical strength of the solution in the case of the acid prepared from the anhydride is hardly appreciable, and will not affect the accuracy of any work in which the solution may be applied as a means of standardization, the solutions made from the purchased product, on the other hand, contain a very appreciable amount of iodine in excess of the theoretical. That iodic acid is somewhat unstable at 30–40° C., gradually losing water with the formation of the anhydride,* is well known, and it is quite possible that to some such gradual change as this must be attributed the fact that the ordinary iodic acid cannot

* Dammer, Anorganische Chemie, i, 564.

be safely employed as a means of standardization unless its purity be directly determined by analysis.

To determine whether a solution of iodic acid, once prepared and standardized, will retain its strength for a long period of time, two such solutions were kept for four months (in the dark) and then again analyzed. The results (averages of several determinations), given in Table II, substantiate the observation of Riegler that a solution of iodic acid will remain of constant strength.

TABLE II.

Constancy of Strength of Iodic Acid Solutions.

Iodic acid Solution.	First analysis. HIO_3 found. grm.	Second analysis. (after four months) HIO_3 found. grm.	Variation. grm.
I	0·1073	0·1072	0·0001—
II	0·1049	0·1046	0·0003—

An approximately one-twentieth normal solution of "chemically pure" sodium thiosulphate was made and its exact strength ascertained by titrating it with standardized iodine. A series of analyses made by oxidizing the sodium thiosulphate to sulphate, and precipitating and weighing as barium sulphate, gave results identical with those obtained with iodine, proving that all the sulphur present in the solution was in the form of thiosulphate. According to Riegler's equation, sodium thiosulphate and iodic acid react molecule for molecule, and solutions of these substances should therefore require for their mutual saturation volumes inversely proportional to their concentration. It was found, however, that when the one-twentieth normal solution of sodium thiosulphate that has been described was titrated in the presence of starch emulsion with an approximately decinormal solution of iodic acid, prepared from the anhydride, a distinctly blue color was produced long before the theoretical amount of iodic acid had been added. It was further noticed that the end-point of the reaction was far from distinct, a faint tinge of blue at first being visible, then suddenly becoming deeper, and immediately reappearing when bleached with sodium thiosulphate. The deficiency in the amount of iodic acid actually required to produce the blue color was not lessened by titrating only three-fourths of the theoretical amount of iodic acid, and estimating the residual thiosulphate with iodine. It was found, however, that the addition of a considerable quantity of potassium iodide to the solution, either before or during the titration, had the marked effect of making the reaction sharp and distinct, entirely preventing the "after separation" of iodine, at the same time

postponing the appearance of the starch blue until a quantity of iodic acid had been added considerably in excess of the theoretical. These experiments were executed with entirely different reagents, and under varied conditions of concentration, the results in every case exactly confirming those already observed.

For the purpose of more particularly investigating this subject, there were prepared and standardized an approximately decinormal solution of sodium thiosulphate, and an approximately one-fiftieth normal solution of iodic acid. Measured portions of the sodium thiosulphate solution were titrated with the iodic acid in the presence of starch emulsion under varying conditions of mass, time and dilution.

To determine the variability of the end-point of the reaction when the titration was conducted as directed by Riegler, a series of experiments was made. Measured amounts of the sodium thiosulphate solution were drawn from a burette into an Erlenmeyer beaker of suitable capacity, the sides of the beaker were carefully washed down with a small amount of water, 5^{cm^3} of starch emulsion were added, and the iodic acid was slowly dropped into the small bulk of acid and starch, with constant agitation of the mixture, until the first tint of blue coloration appeared. The results obtained are given in Table III.

TABLE III.

Variation of the End Reaction between $\frac{N}{10}$ Sodium Thiosulphate and $\frac{N}{50}$ Iodic Acid, in the absence of Potassium Iodide.

	$Na_2S_2O_3$ taken. cm^3.	HIO_3 introduced. cm^3.	Mean value. cm^3.	Variation. cm^3.
(1)	6	28·13		0·19—
(2)	6	27·79		0·53—
(3)	6	28·03		0·29—
(4)	6	28·32	28·32	0·00
(5)	6	28·32		0·00
(6)	6	28·71		0·39+
(7)	6	28·83		0·51+
(8)	6	28·43		0·11+
(9)	4	18·94		0·26+
(10)	4	18·67	18·68	0·01—
(11)	4	18·50		0·18—
(12)	4	18·60		0·08—

These experiments indicate that the constancy of the end reaction in different titrations of equal volumes of the same solution depends to a certain degree on the volume of sodium thiosulphate taken. The results in the case of the maximum amounts vary within a range of $1·04^{cm^3}$, which corresponds to

0·0035 grm. of iodic acid, while the average variation is 0·25$^{cm^3}$, corresponding to 0·0009 grm. The variation in the analyses of the smaller amounts is less, the range being 0·44$^{cm^3}$, corresponding to 0·0015 grm., and the average variation being 0·13$^{cm^3}$, or 0·0005 grm. The probable error which these irregularities would introduce in any series of practical analyses by this method is obviously greater than can ordinarily be permitted in iodometric work.

The experiments detailed in Table IV were performed exactly similarly to those of the last series except that two grams of potassium iodide were added to the sodium thiosulphate before the titration was commenced.

TABLE IV.

Variation of the End Reaction between $\frac{N}{10}$ Sodium Thiosulphate and $\frac{N}{50}$ Iodic Acid, in the presence of Potassium Iodide.

	$Na_2S_2O_3$ taken. cm^3.	HIO_3 introduced. cm^3.	Mean value. cm^3.	Variation. cm^3.
(1)	6	32·53	32·48	0·05 +
(2)	6	32·45		0·03 —
(3)	6	32·67		0·19 +
(4)	6	32·37		0·11 —
(5)	6	32·36		0·12 —
(6)	6	32·50		0·02 +
(7)	4	22·30	22·19	0·11 +
(8)	4	21·98		0·21 —
(9)	4	22·17		0·02 —
(10)	4	22·30		0·11 +

These experiments indicate plainly that in the presence of potassium iodide the end reaction of different titrations of equal volumes of the same solution is practically independent of the amount taken for analysis. The results in the case of the maximum amounts vary within a range of 0·31$^{cm^3}$, or 0·0011 grm. of iodic acid, while the average variation is 0·09$^{cm^3}$, corresponding to 0·0003 grm. The variation in the analyses of the smaller amounts is practically the same as that of the larger, the range being 0·32$^{cm^3}$, corresponding to 0·0011 grm., and the average variation being 0·11$^{cm^3}$, or 0·0004 grm. It is therefore evident that the presence of potassium iodide in the sodium thiosulphate to be titrated will bring the variation of the formation of the reading tint within permissible limits.

A series of experiments was made to determine the nature and effect of the "after coloration" observed to take place when a solution of sodium thiosulphate, free from potassium iodide, was titrated with iodic acid to blue coloration, and then bleached with sodium thiosulphate. The titrations were per-

formed in the usual manner except that the volume was adjusted just before the addition of the iodic acid, and the iodine that was set free after the formation of the first reading tint was destroyed at fixed intervals with measured amounts of sodium thiosulphate. The results are given in Table V.

TABLE V.

Effect of Dilution and Lapse of Time on the "After Coloration."

	$Na_2S_2O_3$ taken. cm³.	HIO_3 introduced. cm³.	$Na_2S_2O_3$ introduced. cm³.						Volume. cm³.
			15 min.	45 min.	1 h. 45 min.	2 h. 45 min.	20 h.	Total.	
(1)	6	27·68	0·25	0·13	0·08	0 00	0·03	0·49	50
(2)	6	27·70	0·20	0·10	0·03	0·03	0·03	0·39	50
(3)	6	28·17	0·16	0·10	0·03	0 01	none.	0·30	50
(4)	6	27·03	0·60	0·26	0·09	0 03	none.	0·98	150
(5)	6	27·60	0·93	0·28	0 06	0·04	0·04	1·35	150
(6)	6	28·60	1·34	0·46	0·17	0·03	0·14	2·14	200
(7)	6	28·85	1·20	0·50	0·28	0 06	0·27	2·31	200
(8)	6	31·63	1·46	0·74	0·10	0·21	0·23	2·74	250
(9)	6	29·90	1·04	0 60	0·23	0·15	0·46	2·48	250
(10)	6	36·09	1·60	1·23	0·63	0·34	0·18	3·98	300
(11)	6	37·59	1·65	1·33	0·72	0·27	0·10	4·07	300
(12)	6	37·23	1·92	1·05	0·64	0·33	*		300

In the experiments with small volumes the evolution of iodine in any considerable quantity ceased after two or three hours, although the solution would become recolored as often as it was bleached for a number of days. The traces of iodine thus set free, however, were seldom equivalent to more than one or two drops of sodium thiosulphate. The larger volumes, however, continued to separate iodine in abundance for a very long time. The amount of iodine thus liberated after the first coloration evidently varies with the amount of iodic acid required for the titration, although not strictly proportional to it. Both of these quantities increase at a regular rate with the volume of the solution.

To show with what accuracy the reaction between sodium thiosulphate and iodic acid may be applied to the direct estimation of one of these substances by the other, the averaged results of a large number of titrations are compared in Table VI. The operations were conducted as directed by Riegler, equal measured volumes of standardized sodium thiosulphate being titrated with iodic acid of known strength, in the presence of starch and under different conditions of time, dilution and mass, the volume of iodic acid required to produce the blue coloration being in each case compared with the volume theoretically required by the terms of Riegler's equation.

* No observation.

TABLE VI.

Titration of $\frac{N}{10}$ Sodium Thiosulphate with $\frac{N}{50}$ Iodic Acid.

	$Na_2S_2O_3$ taken. cm^3.		HIO_3 introduced. cm^3.	HIO_3 required by theory. cm^3.	Error. cm^3.	Error. per cent.	KI present. grm	Volume. cm^3.
(1)	4		18·68	20·32	1·64—	8·0—	—	50
(2)	6		28·32	30·48	2·16—	7·0—	—	50
(3)	6		27·32	30·48	3·16—	7·0—	—	150
(4)	6	*	28·73	30·48	1·75—	6·0—	—	200
(5)	6		30·77	30·48	0·29+	0·01+	—	250
(6)	6		36·97	30·48	6·49+	21·0+	—	300
(7)	6		27·46	30·48	3·02—	10·0—	—	50
(8)	6		26·15	30·48	4·33—	14·0—	—	150
(9)	6	†	26·50	30·48	3·98—	13·0—	—	200
(10)	6		27·16	30·48	3·32—	10·0—	—	250
(11)	6		32·93	30·48	2·45+	8·0+	—	300
(12)	4	*	22·19	20·32	1·87+	9·0+	0·2	50
(13)	6		32·48	30·48	2·00+	7·0+	0·2	50

These results show plainly that the amount of iodic acid required to decompose a given amount of sodium thiosulphate may be considerably above or below that required by the terms of Riegler's equation. Thus, with small volumes, and in the absence of potassium iodide, the thiosulphate is destroyed and the separation of iodine commences when only 93 per cent of the theoretical amount of acid has been titrated. At higher dilutions the action is retarded, so that at 250^{cm^3} very nearly the theoretical amount of acid is required to produce the first blue color, and at 300^{cm^3} an excess of 21 per cent over the theoretical amount must be added. If the "after separation" of iodine is considered to be a measure of the excess of iodic acid, and if its amount is accordingly applied as a correction, it appears that for all volumes below 300^{cm^3} the original thiosulphate is completely destroyed when about 90 per cent of the theoretical amount of iodic acid has been added. The presence of potassium iodide in the system retards the action, so that at small volumes an excess of about 8 per cent of iodic acid must be added to completely destroy the thiosulphate and commence the separation of iodine. It is obvious from the preceding experiments that the reaction between iodic acid and sodium thiosulphate is so indefinite in its nature, and so dependent for its completeness on conditions of time, dilution and mass, that its direct application as a means of standardizing solutions must remain impracticable.

The author is indebted to Professor F. A. Gooch for many valuable suggestions during the course of this investigation.

* HIO_3 added to first blue color.

† Calculated by subtracting from the amount of iodic acid originally titrated, the volume of thiosulphate of equal strength required to bleach the solution after standing twenty hours.

ART. XXVI.—*Solarization effects in Röntgen Ray Photographs;* by WM. LISPENARD ROBB. (With Plates VIII–X.)

IT has long been well known, that in photographing with ordinary light, in cases of over-exposure, the picture upon development may be a positive instead of a negative. This phenomenon is known as solarization, as it is usually produced by over-exposure in strong sunlight. Also, in case of over-exposures not sufficiently long to produce a reversal of the image, the photographic plate may be so affected as to prevent satisfactory development.

Some experiments that I have recently made show that similar effects are produced by the Röntgen rays, and that they have a very important bearing upon the distinctness of photographs taken with these rays, and offer a very simple explanation of the halos that so often appear in such photographs.

The following apparatus was used in these experiments: The induction coil used was a "Thompson Inductorium" as manufactured by the General Electric Co., except that a rotary break was substituted for the one furnished with the coil. The rotary break consisted of a solid brass ring 25^{cm} in diameter and 5^{cm} thick. Two slate quadrants, $2{\cdot}5^{cm}$ thick, were countersunk in the ring. Two copper brushes were arranged so that one was always in contact with the brass and the other alternately with the brass and slate sectors. The ring was mounted on the shaft of a 1 h. p. motor, making 1800 revolutions per minute, and consequently the primary circuit of the induction coil was made and broken 3600 times per minute. A condenser having a capacity of six microfarads was connected with the two brushes. This was the largest condenser available; and it was found that the sparking at the brush, where the circuit was alternately made and broken, was very great, being sufficient to cause great unsteadiness in the illumination of the screen of a fluoroscope. A very simple method was found for overcoming this unsteadiness. A third brush made of mica was placed so as to be in contact with the break and to form an obtuse angle with the brush at which the sparking occurred. This additional brush prevented the sparking; and after it was adopted, the illumination of the fluorescent screen was entirely free from flickering. The current in the primary was adjusted in the following experiments so that the coil would give a spark 25^{cm} long. Single focus vacuum tubes were used. The tubes were spherical in form, about 14^{cm} in diameter, and the distance between the anode and cathode was about 8^{cm}. The tubes used were made by Green & Bauer of Hartford, Con-

necticut, and by the General Electric Co. at their lamp works at Harrison, N. J. In the absence of any exact method of expressing for any given set of apparatus its power of producing Röntgen rays, the statement that the photograph of a portion of a hand reproduced in Plate VIII, fig. 1, was taken with the apparatus just described, will serve to show its character. The time of exposure was 5 seconds and the distance of the hand from the anode 25^{cm}.

Solarization effects were first noticed by the author in connection with the halos surrounding the Röntgen ray photographs of pieces of metal. These halos were in general similar to that shown in Plate VIII, fig. 2, which is a reproduction of the photograph of an aluminum cube, the edge of which was 2·5^{cm}. Surrounding the portion of the photographic plate directly under the cube is a band in which the plate was somewhat affected by the rays. Just outside of this first band is a second one, in which the effect upon the photographic plate was greater than upon any other part of the plate. This halo is easily explained when we consider that in a single focus tube the Röntgen rays come from a considerable area of the anode and that consequently the shadow of the object is surrounded by a penumbra in which the intensity of the radiation increases from the object outward. If the exposure is sufficient to produce solarization, we should have a band in the penumbra where the effect upon the photographic plate would be a maximum exceeding even the effect upon the part of the plate entirely beyond the shadow. The photograph reproduced in Plate VIII, fig. 2, was taken with fifteen minutes' exposure at a distance of 15^{cm}.

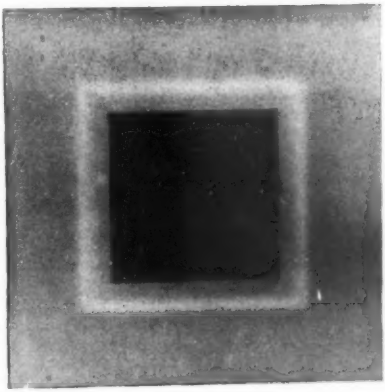
The experiment was repeated with cubes of iron, copper, paraffin, and glass, all of which gave similar halos. In general, the image on the photographic plate was visible to the eye before being placed in the developer—a phenomenon that accompanies solarization when produced by ordinary light.

The following experiments were made in order to prove the correctness of the above explanation and to demonstrate the possibility of a photographic plate becoming solarized by the Röntgen rays.

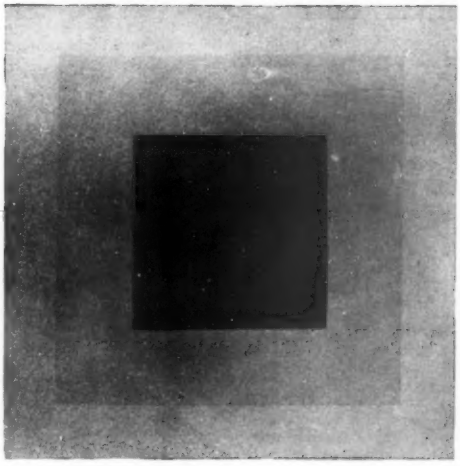
Portions of several plates were covered with pieces of commercial tinfoil and then exposed in succession for different times to the action of the Röntgen rays. It was found that with short exposures a negative was obtained, and with long exposures the image was reversed; the long exposed plates giving a positive when developed. Plate IX, figs. 1 and 2, and Plate X, fig. 1, are reproductions of photographs obtained when a portion of the photographic plate was covered with one layer of commercial tinfoil about 5^{cm} square and 0·0015^{cm} thick, and

1.

2.



1.



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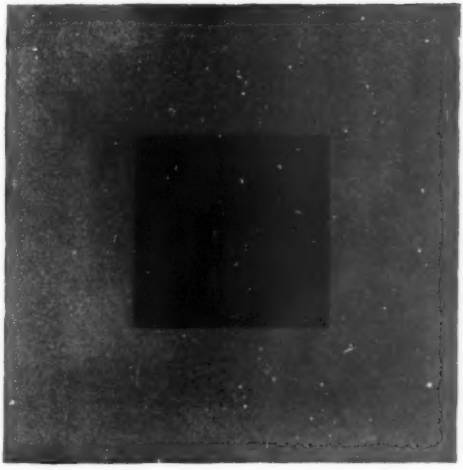
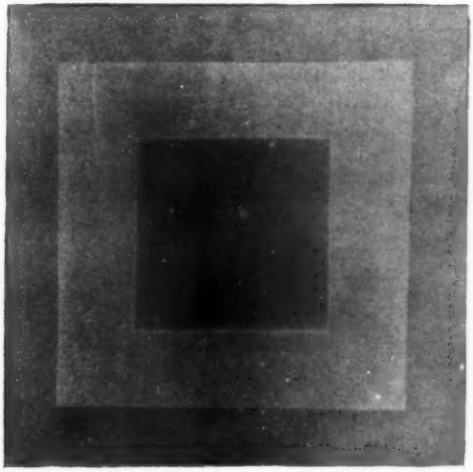
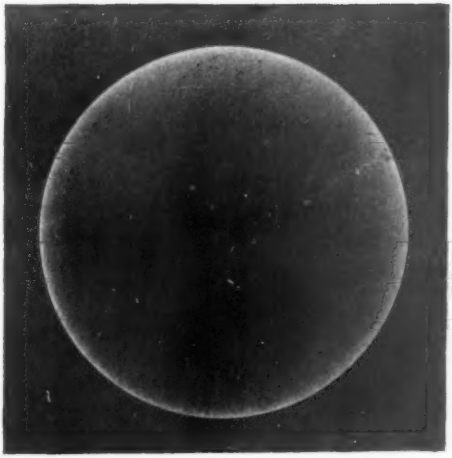


1.



2.



the central portion of this layer was covered with thirty-two additional layers of the same foil about 2·8^{cm} square. Plate IX, fig. 1, shows the result when the plate was exposed for 2·5 minutes; fig. 2, when exposed for 5 minutes, and Plate X, fig. 1, when exposed for 15 minutes. These photographs show that with a short exposure, the portion of the plate covered with a single layer of tinfoil was less affected than the uncovered portion of the plate. When the time of exposure was increased, the shadow of the single layer of tinfoil would only be noticed on the negative by careful inspection, and might easily escape detection. In the case of the longest exposure, where the portion of the plate covered with the single layer of the tinfoil is most affected, we have a reversal of the image and a clear case of solarization.

Experiments were also made with photographic plates partially covered with an aluminum cone having an altitude of 1·25^{cm} and a base 5^{cm} in diameter. When the exposure was short, the uncovered portion of the plate was most affected. When the plate was placed at a distance of 15^{cm} and exposed for 5 minutes, the effect reproduced in Plate X, fig. 2, was obtained. In this case, the portion of the plate under the edge of the cone was most affected. When the time of exposure was increased to 15 minutes, all of the plate covered by the cone was affected more than the uncovered portion, and again we have a reversal of the image and a clear case of solarization.

Seed, Kramer crown, and Carbutts' special X-ray plates, and various developers were tried and found to give similar results. Carbutts' X-ray plates and Carbutts' tabloids for developer were used in making the photographs for the accompanying illustrations.

These experiments seem to prove conclusively the possibility of photographic plates becoming solarized by Röntgen rays. This is interesting as adding one more to the properties possessed in common by these rays and ordinary light. Solarization offers a simple explanation of many of the halo effects observed in Röntgen ray photographs. The most important conclusion to be derived from these experiments is the necessity of carefully timing exposures if we are to obtain good contrasts, much of the indistinctness in Röntgen ray photographs being due to over-exposure rather than to under-exposure.

I desire to express my obligation to Columbia University for the very material assistance given me in carrying on these experiments by placing at my disposal the income of the Barnard Fellowship.

Jarvis Physical Laboratory, Trinity College,
Hartford, Conn., July 24th, 1897.

ART. XXVII.—*The Cape Fairweather Beds; a new marine Tertiary Horizon in Southern Patagonia;* by J. B. HATCHER.

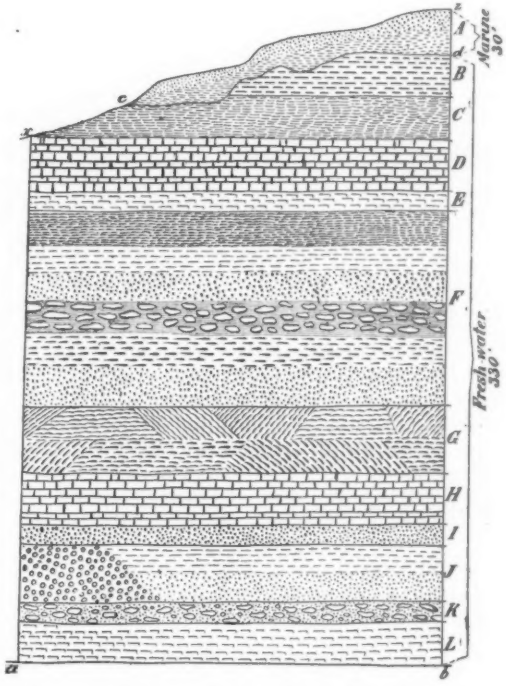
IN July, 1896, the writer discovered near Cape Fairweather, in south latitude about 51° 31′, a series of marine beds with a fairly abundant invertebrate fauna, overlying the fresh water Santa Cruzian beds, which are well represented in this vicinity and contain abundant remains of fossil mammals. It is proposed to call these deposits the Cape Fairweather beds from the name of the cape near which they were first observed.

Going north along the shore, from the mouth of the Gallegos river, the Cape Fairweather beds are first seen at a distance of about two and a half miles, capping the summit of a high tableland on the north side of a rather deep cañon, which empties into the sea from the west. From this point these beds were traced six or eight miles farther north, along the bluffs of the coast; and were seen to extend for some distance westward into the interior, constituting the summit of the higher tablelands. In most places, where present, they are easily recognized by the remains of a large oyster, fragments of which may be seen in great abundance near the summit of the more precipitous bluffs in this vicinity. They are best represented on Rudd's farm, but are also to be seen on the bluff southeast of Mr. Jóse Monte's farmhouse, some six miles inland, though not well displayed.

The Cape Fairweather beds have been deposited upon the eroded surface of the Santa Cruzian beds, as shown by the accompanying section, which was drawn from a sketch made in the field, and represents very accurately the section of the two series of strata on the north side of the cañon above referred to from the top of the tableland, which is reached at *z*, to the level of high tide represented by the line *a*—*b*. The bluff here, as almost everywhere along this coast, is quite perpendicular, and the color, composition and relations of the various strata are easily seen. The irregular line *c*—*d* is the line of contact between the two series of beds and shows well the eroded surface of the lower series upon which the upper beds were deposited.

These new marine deposits are of no very great thickness, so far as observed, only 30 to 40 feet. They consist below of a fine-grained, incoherent sandstone; and above, of a rather coarse, usually loose, but in places, extremely hard conglomerate which passes insensibly into the overlying great Patagonian Shingle formation, from which it can only be distin-

guished by the fossils it contains. Both the sandstones and conglomerates are fairly continuous, but the latter are frequently intruded into the former, and the sandstones sometimes entirely replace the conglomerates. In both, marine invertebrates are quite abundant, and according to Professor Henry A. Pilsbry they point to a Pliocene age for the beds.



These beds are of interest as being the first instance of a marine formation overlying the fresh water Santa Cruzian formation, in regard to the age of which there has been so much doubt; unfortunately, however, they promise to be of little service in determining the age of the latter. It is quite probable that they may aid in correlating certain marine beds of Parana, discovered long ago and referred by Darwin, D'Orbigny and others to the Patagonian beds, but now known to be of much more recent origin. At present I believe them the equivalent of those beds discovered by Darwin in northeastern Tierra del Fuego and provisionally referred by him to the mammalian beds (Santa Cruzian beds)

discovered by Captain Fitzroy at the mouth of the Gallegos river and believed by Darwin to be more recent than the Patagonian beds. As evidence in favor of correlating the Cape Fairweather beds with those reported by Darwin in Tierra del Fuego, I may mention that in the former there are fragments of crab legs very similar to those found in the bluffs of San Sebastian bay; also the fact that all the Tertiary strata in this region dip very gently to the southeast, so that in going from north to south along the coast the different horizons appear at the level of the sea in chronological order. Near the Mt. of Observation, south of the Santa Cruz river, we find at sea level and for some distance above the Patagonian beds overlaid by the Santa Cruzian beds. Farther south, at Coy inlet, the Patagonian beds entirely disappear under the sea, and the Santa Cruzian beds are at the water level, and still farther south at Cape Fairweather they are overlaid by the Cape Fairweather beds; while at San Sebastian bay on the east coast of Tierra del Fuego the Santa Cruzian beds have disappeared below the sea and the Cape Fairweather beds alone are represented. A study of the Cape Fairweather beds may also afford important evidence as to the origin of the numerous salt water lakes in southern Patagonia; and as to the age, origin and distribution of the great Shingle formation of this region. These and other questions will be considered in a more exhaustive paper on the general geology of the country visited.

Princeton University, Aug. 2, 1897.

SCIENTIFIC INTELLIGENCE.

I. Natural History.

1. The *New Series of Contributions from the Gray Herbarium of Harvard University, No. XI*, by Mr. J. M. Greenman, deals with the Mexican and Central American species of *Houstonia*, being a revision of these. It contains also a Key to the Mexican species of *Liabum*, and Descriptions of more than forty new or little known plants from Mexico. Two new genera are added. G. L. G.

2. *Synoptical Flora of North America*, Vol. 1, Part I, Fascicle II, contains critical descriptions of the North American species from *Caryophyllaceæ* to the *Polygalaceæ;* by Asa Gray, LL.D., continued and edited by Benjamin Lincoln Robinson, Ph.D., Curator of the Gray Herbarium of Harvard University, with the collaboration of William Trelease, Sc.D., Director of the Missouri Botanical Garden; John M. Coulter, Ph.D., Professor of Botany in the University of Chicago; and L. H. Bailey, M.Sc., Professor of Horticulture in Cornell University.

A succinct statement which accompanies this welcome publication, shows exactly how the work stands at present. From this we learn that "of the Synoptical Flora, Professor Gray published in 1878 and 1884, two parts including all the Gamopetalous Orders. These parts were reissued by the Smithsonian Institution in 1886, and amount to nearly 1000 imperial octavo pages. For some time before his death Professor Gray, continuing the work, was engaged in monographing the earlier Polypetalous Orders. After the death of Professor Gray the preparation of the Synoptical Flora was carried on by Dr. Sereno Watson, and then by his successor, Dr. B. L. Robinson.

"Following the original plan of the Flora, the treatment of the Polypetalous Orders will form, when completed, Volume I, Part I. Of this portion of the work the first fascicle, comprising the seventeen Orders from Ranunculaceæ to Frankeniaceæ, inclusive, was issued Oct. 10, 1895. The second fascicle now before us carries the work up to Polygalaceæ, and a third, to include the Leguminosæ, is now in preparation by Dr. Robinson."

Botanists appreciate sincerely the careful work which characterizes this joint treatise. Dr. Robinson has spared no pains to keep the Flora on the high plane of Professor Gray's critical investigations, and he has received valuable aid from his distinguished collaborators. We think that the editor has been wise in adhering to the lines laid down by his predecessors. The limitations are here and there occasionally felt perhaps to be rather too strict, but the result has been on the whole far more satisfactory than would have been a complete or even partial overturn. Dr. Robinson and his coadjutors are carrying out the plan in a manner which must commend itself to all who know the circumstances of the case. G. L. G.

3. *An Illustrated Flora of the Northern United States, Canada, and the British Possessions*, from Newfoundland to the Parallel of the Southern Boundary of Virginia, and from the Atlantic Ocean westward to the 102d Meridian; by NATHANIEL LORD BRITTON, Ph.D., Emeritus Professor of Botany in Columbia University, and Director in Chief of the New York Botanical Garden, and Hon. ADDISON BROWN, President of the Torrey Botanical Club. The descriptive text chiefly prepared by Professor Britton, with the assistance of specialists in several groups; the figures also drawn under his supervision. In three volumes. Vol. II. Portulacaceæ to Menyanthaceæ. Portulaca to Buckbean. Charles Scribner's Sons. 1897.

The first volume of this work has been already noticed in this Journal. To what was then said, nothing need now be added in regard to the second volume, except further congratulations to the authors on their success in giving to Botanists a useful treatise at a very reasonable price. They maintain in the present volume the high character of typographical execution which made the first volume so attractive. The work is progressing steadily; the final volume being promised for early winter. G. L. G.

4. *Guide to the Genera and Classification of the North American Orthoptera found north of Mexico;* by SAMUEL H. SCUDDER. (Cambridge, Edw. H. Wheeler), pp. 1–89, 1897.—This convenient little set of tables for the identification of Orthoptera was prepared for the use of students, and is but preliminary to a fuller general work on the classification of the group. Although containing references only to data already published or about to be published, the tables include nearly two hundred genera.

5. *Das Tierreich. Eine Zusammenstellung und Kennzeichnung der rezenten Tierformen. 1 Lief. Aves. Redakt., A. Reichenow. Podargidæ, Caprimulgidæ u. Macropterygidæ;* bearb. von ERNST HARTERT, pp. 1–98, figs. 1–16. Berlin, 1897. (R. Friedländer & Sohn.)

II. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *American Association for the Advancement of Science.*—The forty-sixth meeting of the American Association was held at Detroit, from August 9 to 14. The President of the meeting was Dr. Wolcott Gibbs of Newport. The senior vice-president, Prof. Theodore Gill of Washington, who took the place of the retiring president, the late Prof. E. D. Cope, delivered an able address upon Prof. Cope's life and work. Addresses were also delivered by the vice-presidents of the several sections. Nearly three hundred members and associates were in attendance. The list of papers was considerably larger than at the last meeting. The fact that the British Association was to assemble at Toronto on August 18 gave especial interest to the occasion.

The place selected for the next meeting of the Association—its fiftieth anniversary—is Boston and Prof. F. W. Putnam was

elected President. The Vice-Presidents chosen for the several sections are as follows: Section A, E. E. Barnard, of Chicago; Section B, Frank P. Whitman, of Cleveland; Section C, Edgar F. Smith, of Philadelphia; Section D, M. E. Cooley, of Ann Arbor; Section E, H. L. Fairchild, of Rochester; Section F, A. S. Packard, of Providence; Section G, W. G. Farlow, of Cambridge; Section H, J. McKeen Cattell, of New York City; Section I, Archibald Blue, of Toronto. Mr. L. O. Howard, of Washington, D. C., was elected Permanent Secretary.

The following is a list of the papers accepted for reading:

SECTION A. *Mathematics and Astronomy.*

R. S. WOODWARD: Modification of Eulerian cycle due to inequality of the equatorial moments of inertia of the earth. Integrations of the equations of rotation of a non-rigid mass for the case of equal principal moments of inertia.

A. MACFARLANE: A new method of solving certain differential equations that occur in mathematical physics. The theory of quadratic equations.

T. H. SAFFORD: Psychology of the personal equation.

G. A. MILLER: The simple isomorphism of a substitution group into itself.

ARTEMAS MARTIN: On rational right triangles. No. I.

VIRGIL SNYDER: Condition that the line common to n-1 planes in an x-space may lie on a given quadric surface in the same space.

J. B. SHAW: Communicative metrices.

H. B. NEWTON: Continuous groups of spherical transformations in space.

A. G. GREENHILL: Stereoscopic views of spherical catenaries and gyroscopic curves.

F. H. BIGELOW: The importance of adopting standard systems of notation and coördinates in mathematics and physics.

L. A. BAUER: On the secular motion of the earth's magnetic axis. Simple expressions for the diurnal range of the magnetic declination.

R. D. BOHANNAN: Remarkable complete quadrilateral among the Pascal lines of an inscribed six-point of a conic.

J. W. GLOVER: General theorems concerning a certain case of functions deduced from the properties of the Newtonian potential function.

W. H. METZLER: Compound determinants.

W. S. AUCHINCLOSS: Waters within the earth, and laws of rainflow.

E. O. LOVETT: The theory of perturbations and Lie's theory of contact transformations.

JAMES MCMAHON: Some results in integration expressed by the elliptic integrals.

W. F. DURAND: The treatment of differential equations by approximate methods.

SECTION B. *Physics.*

F. P. WHITMAN and MARY C. NOYES: Effect of heat on the elastic limit and ultimate strength of copper wire.

A. L. FOLEY: Arc spectra.

C. F. BRUSH: Transmission of radiant heat by gases at varying pressures. Measurement of small gaseous pressures.

D. C. MILLER: Electrical conductivity of certain specimens of sheet glass with reference to their fitness for use in static generators.

W. A. ROGERS: Final determination of the relative lengths of the Imperial yard of Great Britain and the *mètre des archives*.

S. J. BARNETT: Influence of time and temperature on the absolute rigidity of quartz fibers.

C. D. CHILD: Discharge of electrified bodies by X-rays.

F. P. WHITMAN: On the brightness of pigmented surfaces under various sources of illumination.

H. S. CARHART: The design, construction and test of a 1250-Watt transformer.

K. E. GUTHE: Electrolytic action in a condenser.

H. T. EDDY: Graphical treatment of alternating currents in branch circuits in cases of variable frequency.

A. MACFARLANE: On simple non-alternating currents.

L. A. BAUER: Magnetic survey of Maryland

R. L. LITCH: New method of determining the specific heat of liquids.

G. L. MOLER and F. BEDELL: Instruments for determining the frequency of an alternating current.

W. J. HUMPHREYS: Effect of pressure on the wave-lengths of the lines of the emission spectra of the elements.

C. L. NORTON: New form of coal calorimeter.

C. R. CROSS: Notes on the recent history of musical pitch in the U. S.

F. A. LAWS: New form of harmonic analyzer.

N. E. DORSEY: Determination of the surface tension of water, and of certain aqueous solutions, by means of the method of ripples.

F. H. BIGELOW: Series of international cloud observations made by the U. S. Weather Bureau, and their relation to meteorological problems

C. F. MARVIN: Kites and their use by the Weather Bureau in explorations of the upper air.

B. B. BRACKETT: Effects of tension and quality of the metal upon the changes in length produced in iron wires by magnetization.

E. W. MORLEY and D. C. MILLER: On the coefficient of expansion of certain gases.

E. F. NICHOLS: Note on the construction of a sensitive radiometer.

E. L NICHOLS and E. MERRITT: Photograph of manometric flames.

G. W. PATTERSON, JR.: Electrostatic capacity of a two-wire cable

C. P. STEINMETZ; Screening effect of induced currents in solid magnetic bodies in an alternating field.

CARL BARUS: Rate at which hot glass absorbs superheated water. Method of obtaining capillary canals of specified diameter.

F. BEDELL, R. E. CHANDLER and R. H. SHERWOOD, JR.: Predetermination of transformer regulation.

W. R. WHITNEY: An electrical thermostat.

W. O. ATWATER and E B. ROSA: Apparatus for testing the law of conservation of energy in the human body.

E. B. ROSA and A. W. SMITH: Electrical resonance and dielectric hysteresis.

MARGARET E MALTBY: Method for the determination of the period of electrical oscillations and other applications of the same.

C. P. MATTHEWS: Two methods of measuring mean horizontal candle power.

SECTION C. *Chemistry.*

A. B. PRESCOTT: Alkyl bismuth iodides. Kola tannin.

J. U. NEF: Chemistry of methylene.

P. C. FREER: Action of sodium on methylpropylketon and on acetophenon. Constitution of some hydrazones.

A. W. BURWELL: Decomposition of heptane and octane at high temperatures.

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* Including papers read before the Association of Economic Entomologists.

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C. A. DAVIS: *Trillium grandiflorum* (Michx.) Salisb.; its variations, normal and teratological.

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W. H. HOLMES: Surveys of ancient cities in Mexico. Archæological researches in the Trenton gravels.

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H. B. KUMMEL: Implement-bearing sand deposits at Trenton, N. J.

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H. C. MERCER: Report of an examination of the trenches dug on the Lalor farm, July 25–29.

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2. *The Transactions of the American Microscopical Society*, volume xviii.—This volume contains the report of the nineteenth annual meeting held at Pittsburgh in August, 1896, and gives in full a large number of papers with the discussions which they called out. The presidential address by A. Clifford Mercer discusses the experimental study of aperture as a factor in microscopic vision, and is accompanied by a series of plates giving reproductions of photomicrographs.

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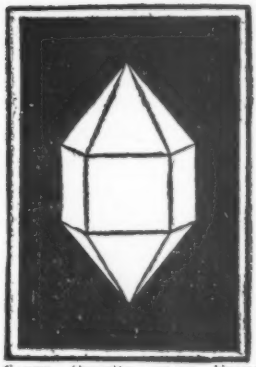

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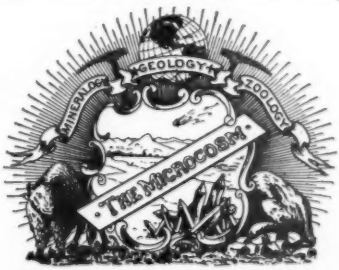

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(1) Described in this Journal Sept., 1895. (2) Ibid Nov., 1887. (3) Proc. Roch. Acad. Sci. Vol. 1. (4) This Journal Aug., 1888. (5) Ibid July, 1893. (6) Ibid June, 1894.

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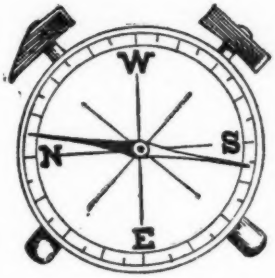

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